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THE TRANSACTIONS

OF THE

SOUTH AFRICAN

PHILOSOPHICAL SOCIETY.

VOLUME V, PART II.

1886—1889.

28830

CAPE TOWN:
PRINTED FOR THE SOCIETY BY MURRAY & ST. LEGER.
1893.

COUNCIL

OF THE

South African Philosophical Society

FOR 1886—1887.

H. Bolus, F.L.S., *President.*
Hon. C. A. Smith, M.A., *Treasurer.*
W. H. Finlay, M.A., F.R.A.S., *General Secretary.*
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Hon. J. X. Merriman, M.L.A.
L. Péringuey, F.Z.S.
R. Trimen, F.R.S., F.L.S., F.Z.S.

FOR 1887—1888.

W. H. Finlay, M.A., F.R.A.S., *President.*
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R. Marloth, Ph. D., M.A.
J. X. Merriman, M.L.A.
L. Péringuey, F.Z.S.
R. Trimen, F.R.S., F.L.S., F.Z.S.

FOR 1888—1889.

W. H. Finlay, M.A., F.R.A.S., *President.*
Hon. C. A. Smith, M.A., *Treasurer.*
David Gill, LL.D., F.R.S., F.R.A.S., *General Secretary.*
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F. Guthrie, LL.B.
R. Marloth, Ph.D., M.A.
Hon. J. X. Merriman.
P. MacOwan, B.A., F.L.S.
L. Péringuey, F.Z.S.
R. Trimen, F.R.S. F.L.S., F.Z.S.

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ERRATA.

VOLUME V. PART II.

Page 229, in title, for ACANTHOSIOYOS read ACANTHOSICYOS.

„ 229, line 7, for *Acanthosioyos* read *Acanthosicyos*.

„ 230, line 18, for rolaquint read coloquint.

„ 267, line 27, for *raffra* read *caffra*.

„ „ „ for *spinoza* read *spinosa*.

„ „ line 28, for *ferax* read *ferox*.

„ 268, line 5, for glameroles read glomerules.

„ „ line 16, for *stolanifera* read *stolonifera*.

„ „ line 21, for *detineus* read *detinens*.

„ „ line 26, for *detineus* read *detinens*.

„ 269, line 31, for *haematosylon* read *haematoxylon*.

„ „ line 36, for *Oaffra* read *Caffra*.

„ 271, line 3, for *eriolaba* read *erioloba*.

„ 272, line 21, for Fir read The.

„ 273, line 31, for *destineus* read *detinens*.

MINUTES OF PROCEEDINGS.

Ordinary Monthly Meeting.

TUESDAY, AUGUST 24, 1886.

VEN. ARCHDEACON LIGHTFOOT, B.D., IN THE CHAIR.

The receipt was announced of a pamphlet by Dr. Emil Cohen (corresponding member of the Society) entitled "Ueber die von den Eingeborenen Süd-Afrikas verwendeten Producte des Mineralreichs" and the thanks of the Society were voted to the donor.

An extract was read from a letter from the Rev. G. H. R. Fisk respecting the tortoises he had sent to England. Two of them were found to be new to science.

Mr. Péringuey said he had found a new tortoise in Namaqualand, and Dr. Marloth had sent him some from Damaraland of which four were undoubtedly new.

Dr. Gibbon's list of South African Mollusca was presented to the Society.

Mr. Finlay announced his discovery of Winnecke's periodical Comet on August 19.

Dr. Shaw exhibited a petrified branch picked up at Hout's Bay, and afterwards read his paper on the "Sand Glaciers in S. Africa." He prefaced it by an account of the sand-dunes on the East Coast of England and West of Scotland.

Mr. J. C. Silberbauer described the petrification of a large forest in California, 1,000 feet above sea level.

Mr. Péringuey said that some sand-glacier formation was to be found at Kalk Bay.

Mr. Sawerthal had seen specimens, like the one exhibited by Dr Shaw, in the Drakensberg.

The thanks of the Society were voted to Dr. Shaw.

Mr. Woods gave an account of the photographic observations to be made at the Royal Observatory on the occasion of the Eclipse of the Sun on August 29.

Ordinary Monthly Meeting.

TUESDAY, SEPTEMBER 28, 1886.

MR. F. F. RUTHERFOORD, IN THE CHAIR.

Mr. Advocate Innes and Mr. T. J. Anderson were duly elected ordinary members of the Society.

Dr. Gill reported the discovery, by Mr. Finlay, of a Comet on Sunday, Sept. 26, 1886.

Mr. Péringuey reported an important discovery he had made regarding the habits of the *Phylloxera Vastatrix*. He had found that when the weather became cold the false female assumed a leaden hue and became motionless, but revived on being subjected to a high temperature, but did not lay. This was very important considering the difficulty that had been experienced in obtaining a sufficient supply of Bisulphide of Carbon, inasmuch as it shewed that there could have been no spread of the disease during the winter months. He had now a sufficient quantity of Bisulphide of Carbon to commence the spring campaign.

Mr. A. G. Howard then read a paper on "The Value of Cape Point as a Warning Station for Meteorological Changes."

Dr. Gill, Dr. Marloth, and the Rev. G. H. R. Fisk made some remarks on the subject, and the meeting closed with the usual votes of thanks.

Ordinary Monthly Meeting.

WEDNESDAY, OCTOBER 27, 1886.

MR. H. BOLUS, F.L.S., PRESIDENT, IN THE CHAIR.

Mr. Advocate Leonard, Q.C., Mr. Advocate P. Molteno, Mr. L. Michell, and Capt. Hewat were duly elected ordinary members of the Society.

The Secretary read Mr. McNellan's paper on "Van Wyk's Vlei Reservoir."

The Rev. G. Stegmann asked whether there was likely to be any danger of the dam silting up. He strongly advocated the making of similar works in the district of Oudtshoorn.

Mr. Merriman was glad to believe the reservoir was a complete success. It had been made at comparatively little cost, holding

35,000 million gallons and costing £20,000. The Cape Irrigation Act had been copied in Victoria with great success, but had failed here from want of combination among the people.

Prof. MacOwan referred to the reports that had been prevalent that the soil at Van Wyk's Vlei was so brack that nothing would grow there, and said that this was far from being the case, and that there was no necessity for planting salt bushes, &c.

Mr. J. C. Silberbauer referred to the dreadful drought that had prevailed in the district about Van Wyk's Vlei for the last four years. He had been informed that the Beaufort Dam was being rapidly silted up.

Mr. Bolus was of opinion that the question of silting up was one of the most important to be considered in the making of dams. He thought similar work should be carried out near populous centres *e.g.*, on the Sunday's, the Vaal, and the Fish Rivers.

Mr. Merriman said that there was a special machine which was very efficacious in clearing out silt.

Dr. Marloth then read a paper on "Nara," a plant which furnishes food for the natives about the neighbourbood of Walfisch Bay.

Mr. Bolus said many efforts had been made in Europe to cultivate this plant—at Kew amongst other places. In each case the seeds germinated and the plant grew two or three inches and then died.

Dr. Marloth replied to a number of questions from various members, and the meeting closed with votes of thanks to Dr. Marloth and Mr. McNellan for their very interesting papers.

Ordinary Monthly Meeting.

WEDNESDAY, NOVEMBER 24, 1886.

MR. H. BOLUS, F.L.S., PRESIDENT, IN THE CHAIR.

Mr. Péringuey exhibited a remarkable instance of mimicry in animals—a crab covered with seaweed—which he had received from Colonel Bowker. The sea-weed was trimmed in such a manner as to afford a hiding-place for the crab while he was waiting for his prey.

Dr. Gill then gave an account of the measurement of a base-line in the neighbourhood of Port Elizabeth by the Trigonometrical Survey party, prefacing his account by an explanation of the means taken to obtain a complete survey of the Colony.

Mr. Finlay read a short note on the Orbit of the Comet discovered by him on September 26, 1886.

Dr. Gill said the discovery of this Comet was a very important discovery, and one that would excite much interest in astronomical circles.

Ordinary Monthly Meeting.

Wednesday, January 26, 1887.

Mr. H. Bolus, F.L.S., President, in the Chair.

The Rev. B. P. Marchand, B.A., and Mr. James Easton were duly elected ordinary members of the Society.

The following donations of books were announced and the thanks of the Society voted to the donors :

Annual Report of the Department of Mines, New South Wales, 1885.

Bulletin de la Société Impériale des Naturalistes de Moscou, 1886, No. 1.

Annalen des K. K. Naturhistorischen Hofmuseums, Band I., No. 2.

Boletin de la Academia Nacional de Ciencias en Cordoba, Vol. VIII., part 4.

Journal of the Cincinnati Society of Natural History, Vol. IX., parts 2 and 3.

Colonial and Indian Exhibition, 1886—Descriptive Catalogue of a Collection of Economic Minerals of Canada, by the Geological Corps.

List of Donations to the Bodleian Library in 1885.

Report of the Trustees of the Australian Museum for 1885.

Prof. Guthrie exhibited a specimen of quartz from the Cape Flats.

The Rev. G. H. R. Fisk exhibited a snake from Touws River which was new to him and to Mr. Trimen.

Mr. Péringuey thought it a very young specimen, and that it might be an Elaps.

Prof. MacOwan sent for exhibition specimens received from Dr. Duminy of the bark of trees which was said to be an infallible antidote for the poison of snake-bite.

Mr. Péringuey described some experiments he had made on the potency of snake-poison. He did not believe that anyone had ever recovered from the bite of a colubrine snake.

Mr. Bolus suggested that Mr. Péringuey and Dr. Marloth should make some experiments on this alleged antidote and these gentlemen expressed their willingness to do so.

Mr. Finlay gave a short account of the large Comet which was visible in the South-West. He stated that its path showed it was a member of a system of comets which pass very close to the sun, a system which includes the Comets 1843 I., 1880 I., and 1882 II.

Ordinary Monthly Meeting.

WEDNESDAY, MARCH 21, 1887.

MR. H. BOLUS, F.L.S., PRESIDENT, IN THE CHAIR.

The following presents were announced and the thanks of the Society voted to the donors :

Annuario del Observatorio Astronomico Nacional de Tacubaya (Mexico) 1887.

Bulletin de la Société Impériale des Naturalistes de Moscou, 1886, No. 3.

Journal and Proceedings of the Royal Society of New South Wales, Vol. XIX, 1885.

Notes on Insects apparently of the genus Margarodes, Lansd. Guilding, by R. Trimen, F.R.S.

Mr. Trimen exhibited some ground pearls from ants' nests. Similar shells from the West Indies sixty years ago were sent to the British Museum. Mr. Trimen had secured three specimens of the insect (*coccus*), and it was peculiar as having no mouth. Very little is known about the insect at present.

Mr. Ponder exhibited some microscopic photographs taken by himself.

Mr. Bolus then read his "Notes, Chiefly Botanical, on the country between Delagoa Bay and Barberton."

Dr. Marloth said that several of the plants mentioned by Mr. Bolus had been found by him in Damaraland.

An interesting discussion followed, in which Messrs. Péringuey, Trimen, Merriman, and Dr. Murray took part.

The meeting closed with an unanimous vote of thanks to Mr. Bolus for his paper.

Ordinary Monthly Meeting.

WEDNESDAY, MARCH 30, 1887.

MR. H. BOLUS, F.L.S., PRESIDENT, IN THE CHAIR.

The following presents were announced and the thanks of the Society voted to the donors :

From Smithsonian Institution :

Smithsonian Report, 1884, 2 vols.

Proceedings of the American Philosophical Society, Nos. 96–123.

Laws and Regulations of the American Philosophical Society.

Register of Papers published in the Transactions and Proceedings of the American Philosophical Society.

List of Members of the American Philosophical Society from its formation to March 15, 1880.

List of Surviving Members of the American Philosophical Society on March 5, 1886.

Journal of the Cincinnati Society of Natural History, Vol. VIII., Nos. 1–4, Vol. IX., No. 1.

Bulletin of the California Academy of Sciences, Vol. II., No. 5.

The Rev. G. H. R. Fisk exhibited a snake caught near the Black River—probably a young specimen of the " Cross Snake."

He also read a few notes on the effect of the sting of the scorpion, with especial reference to the alleged death of a girl at Hopefield. He believed that the girl had probably been bitten by some other animal, *e.g.*, a horned adder, and cited several instances of people who had been stung by scorpions without serious result.

Mr. Trimen said that the only other animal besides a snake that could have inflicted the double bite in this case was one of the large spiders.

Ordinary Monthly Meeting.

WEDNESDAY, MAY 4, 1887.

MR. H. BOLUS, F.L.S., PRESIDENT, IN THE CHAIR.

The following donations were announced and the thanks of the Society voted to the donors.

Jahrbucher der K.K. Central-Anstalt fur Meteorologie and Erdmagnetismus, 1885.

Journal of the Cincinnati Society of Natural History, Vol. IX., No. 4.

Memoirs of the Literature College, Imperial University of Japan, No. 1.

Mr. Péringuey exhibited a snake, *Homolosoma lutrix*, with two heads, which was alive when it reached him, and was able to put out two tongues.

Mr. Howard read a paper on "The Storms of South Africa."

The author had found that none of the usual theories proposed would suit the facts as observed at the Cape, and had endeavoured to develope a theory from the results of the 8 a.m. simultaneous meteorological observations.

The Secretary read Dr. Bachmann's letter on the supposed fatal effects of a scorpion's bite.

Mr. Fisk said that all authorities who had studied the subject of scorpions had never discovered any other poison than that connected with the sting in the tail.

Mr. Trimen was of opinion that the evidence in the two cases was very weak: it did not require much knowledge of Zoology to know that the nippers could not be used for inflicting a venomous wound.

Mr. Péringuey said that he had quite recently examined a scorpion with great care, but could find no aperture in the nippers.

Mr. Péringuey gave an account of his investigations on the development of the *Phylloxera vastatrix* at the Cape.

Last year the first winged insect was observed at the end of April, this year in January. A most important question was—would the cold at the Cape be sufficient to cause Phylloxera to hibernate? He found that although the mean temperature did not fall to 47° F., yet there was a semi-hibernation at the Cape.

A vote of thanks to Messrs. Howard and Péringuey closed the proceedings.

Ordinary Monthly Meeting.

WEDNESDAY, MAY 25, 1887.

MR. H. BOLUS, F.L.S., PRESIDENT, IN THE CHAIR.

The following donations were announced and the thanks of the Society voted to the donors:

Victorian Year Book, 1885-86.

Journal of Cincinnati Society of Natural History, Vol. X., 1.

Vierzehnter Bericht des Museums für Volkerkunde in Leipzig, 1886.

Mitteilungen des vereins für Erdkunde zu Leipzig, 1883, pts. 1, 2, 1884, 1885.

The Rev. G. H. R. Fisk exhibited a snake from Namaqualand (*coronella cana*), of a different colour from the ordinary ones ; it was salmon-coloured underneath. He also drew attention to some remarks that had appeared in "Nature" during the present year, that the poison of a snake had no effect upon another snake of the same species, and quoted a case that had come under his personal observation, in which one horned snake (*vipera cornuta*) had bitten another of the same species, with the result that the attacking snake died, apparently from exhaustion, whilst the one bitten remained unaffected.

Dr. Shaw exhibited an otolith of a whale found on Robben Island.

Prof. MacOwan exhibited an African variety of *alectoria jubara* found near Smithfield, O.F.S.

Dr. Beck then read his paper on "Physiology Teaching in Schools."

An exceedingly interesting discussion ensued in which Messrs. Merriman, Fisk, MacOwan, Bolus, and Sir J. H. de Villiers took part. All the speakers with the exception of Mr. Merriman agreed with Dr. Beck as to the advisability of introducing the study of Elementary Physiology into schools. Mr. Merriman was of opinion that more harm than good would be done by teaching young lads a smattering of this subject at an age when they ought to be studying History and Geography.

A vote of thanks to Dr. Beck closed the proceedings.

Ordinary Monthly Meeting.

WEDNESDAY, JUNE 29, 1887.

MR. H. BOLUS, F.L.S., PRESIDENT, IN THE CHAIR.

Dr. Marloth read a paper on "The Adulteration of Brandy," and in connection with it he exhibited a number of specimens of brandies containing various amounts of fusel oil.

An interesting discussion followed the reading of the paper.

Mr. Easton desired to know whether the brandy containing fusel oil would not fall under the penalties of Act No. 28 of 1883.

Mr. Sivewright enquired whether age would not remove the fusel

oil, and Dr. Marloth explained that this might be the case to a certain limited extent but he had no actual experience of such changes.

Prof. MacOwan gave an account of the brandy distilled by Messrs. Parkes Brothers of Wheatlands in the Graaff-Reinet district. He thought it probable that the new oak casks in which the brandy was put absorbed some of the alcohol of the *amyl* series, and thus improved its quality.

The Rev. G. H. R. Fisk referred to the connection of the low-class brandies with the prevalence of crime, and thought the Legislature should take steps in the direction pointed out by Dr. Marloth.

A vote of thanks was then conveyed to the latter gentleman for his interesting paper.

Ordinary Monthly Meeting.

WEDNESDAY, JULY 27, 1887.

MR. H. BOLUS, F.L.S., PRESIDENT, IN THE CHAIR.

The following books were announced as having been presented to the Society and the thanks of the Society were voted to the donors :

Journal of the Cincinnati Society of Natural History, Vol. X., 2.

Meteorologische Beobachtungen am Met. Obs. der Landwirtschaftlichen Akademie bei Moskau.

Bulletin de la Société Impériale des Naturalistes de Moscou, 1886 No. 4 and 1887 No. 1.

Dr. J. A. Ross was elected an ordinary member of the Society.

Dr. Gill read his lecture on "The Applications of Photography in Astronomy." In the course of it he gave an account of the proceedings of the Photographic Conference held at Paris in April last, and the resolutions passed by the large body of astronomers there assembled with reference to the construction of Stellar Charts.

The Hon. C. A. Smith remarked that a wide field for investigation was opened with respect to the photographic magnitudes of stars as compared with telescopic magnitudes ; and that it would be interesting to take a photograph of the Coal-sack with very long exposure.

Dr. Atherstone called attention to the date as being the Jubilee of Telegraphy. Fifty years ago he was present at the Academy of Sciences, Paris, when Daguerre brought forward his discoveries in Photography, and Morse his Telegraphic system.

A vote of thanks to Dr. Gill closed the proceedings.

Annual General Meeting.

Wednesday, August 17, 1887.

Mr. H. Bolus, F.L.S., President, in the Chair.

The reports of the Secretary and Treasurer were read and adopted.

The Members present proceeded to the election of President and Council for the ensuing year, with the following result :—

President : Mr. W. H. Finlay, M.A., F.R.A.S.

Members of Council : Mr. J. H. M. Beck, M.B.
Mr. H. Bolus, F.L.S.
Mr. D. Gill, LL.D., F.R.S., F.R.A.S.
Prof. Guthrie, LL.B.
Mr. R. Marloth, M.A., Ph.D.
Hon. J. X. Merriman, M.L.A.
Prof. MacOwan, B.A., F.L.S.
Mr. L. Peringuey, F.Z.S.
Hon. C. A. Smith, M.A.
Mr. R. Trimen, F.R.S., F.L.S., F.Z.S.

Mr. Bolus delivered the Presidential address on "The Flora of the Cape Peninsula."

REPORT ON THE PROCEEDINGS OF THE SOUTH AFRICAN PHILOSOPHICAL SOCIETY

DURING THE YEAR ENDING 1887, JULY 31.

1. Since the last Annual General Meeting eleven Ordinary Meetings have been held. The average number of members present has been fourteen, and of visitors eight, making an average total attendance of twenty-two.

2. At the Ordinary Meetings fourteen papers have been read in the subjects Geology, Zoology, Botany, Engineering, Astronomy, Geodesy and Meteorology. Notes and shorter communications on a variety of subjects have also been brought before the Society ; brief accounts of these will be found in the Notes of Proceedings.

3. Thirty-two presents of books have been received during the year ; Fifty-eight copies of the Transactions of the Society have been distributed to learned Societies in Europe, America and Australia.

4. During the year nine Ordinary Members have been elected, and eight have resigned. The total number of Ordinary Members is sixty-eight, and of Corresponding Members twenty three.

5. Volume IV. of the Society's Transactions is ready for press, and the printing of it will be commenced in a short time.

W. H. FINLAY,
General Secretary.

THE TREASURER in Account with the South African Philosophical Society for the year ended 30th June, 1887.

1886.		£	s.	d.	£	s.	d.	1886		£	s.	d.	£	s.	d.
July 1	To Balance :							July.	By Printing Account ...		...		5	19	1
	Mortgage Bond ...	200	0	0					,, Clerical Assistance ...		...		32	0	0
	Bank Balance ...	16	19	4					,, Petty Expenses ..	4	13	6			
					216	19	4		,, Commission on Collection						
	,, Subscriptions, Arrear	12	0	0					of Subscriptions ...	0	16	0			
	Current	106	0	0					,, Carriage of books ...	1	0	0			
	Overpayment on												6	9	6
	Subscriptions ...	0	2	0					,, Rent of Side Room of						
					118	2	0		Exchange		...		15	0	0
	,, Interest on Bond to														
	31st Dec., 1886 ...				14	0	0								
	,, Sale of Transactions				1	4	0	1887	,, Balance :				59	8	7
								June 30.	Mortgage Bond ...	200	0	0			
									Bank Balance, as per						
									Pass Book... ..	90	16	9			
													290	16	9
					350	5	4						350	5	4

Cape Town, 26th July, 1887.

C. ABERCROMBIE SMITH, Hon. Treasurer.

We, the undersigned, Members of the South African Philosophical Society, hereby certify that we have examined the above account, compared the cash payments with the vouchers and the balance with the Pass Book and Mortgage Bond, and found the same correct.

Cape Town, 26th July, 1887.

JOHN E. B. ROSE,
L. MARQUARD. } Auditors.

MINUTES OF PROCEEDINGS.

Ordinary Monthly Meeting.

WEDNESDAY, AUGUST 31, 1887.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

Mr. J. Andrews and Dr. R. N. Hormazdje (Howard) were duly elected Ordinary Members of the Society.

The Rev. G. H. R. Fisk read a short note referring to a return by Sir J. Fayrer in which the phrase scorpion-bite occurred, and also a note from Sir J.F. saying that this was of course a mistake and should be scorpion-sting.

Mr. Fisk exhibited five specimens of *Testudo Trimeni*, the first that had reached him since the tortoise had been named. This species of tortoise was first exhibited and its peculiarities pointed out by Mr. Fisk in May, 1884.

Professor MacOwan explained the method of arranging in trays the specimens in the Government Herbarium.

Mr. Péringuey read a paper on "The Economy of Insect-Life in reference to the Fertilization of Plants," and exhibited a number of specimens of flies with long rostra specially suited for flowers with deep nectaries.

Prof. MacOwan bore testimony to the value of this paper and described the process of vegetative reproduction of the Disa Grandiflora.

Mr. Finlay read a paper on "Approximate Tide Constants for Table Bay and Algoa Bay," pointing out that the tables at present in use were 24 minutes in error.

Ordinary Monthly Meeting.

WEDNESDAY, SEPTEMBER 28, 1887.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

The Rev. H. M. Foot, Hon. J. W. Sauer, and Mr. Advocate H. B. Sauer were duly elected Ordinary Members of the Society.

M. A. G. Howard read a paper on "The Reduction of Barometer Readings to Sea-level." The writer had come to the conclusion that the usual formulæ could not be used for deducing isobars in such a country as South Africa, and he had substituted for them reductions which did not depend on the temperatures at the various stations.

Messrs. Gill, Abercrombie Smith and Finlay, while admitting the uncertainties in the ordinary method of deducing the temperature of the air column between two places, were of opinion that Mr. Howard was wrong in neglecting this temperature altogether.

Mr. Murphy, electrician on the Eastern and South African Telegraph Company's Ship "Great Northern," gave an account of the methods of testing and repairing sub-marine cables, and exhibited specimens of the different styles of effecting a joint in a severed cable.

A number of questions were asked by the members in connection with the subject, to which Mr. Murphy replied.

A vote of thanks to Messrs. Howard and Murphy closed the proceedings.

Ordinary Monthly Meeting.

WEDNESDAY, OCTOBER 26, 1887.

MR. H. BOLUS, F.L.S., VICE-PRESIDENT, IN THE CHAIR.

The following donations were announced and the thanks of the Society voted to the donors :

Bulletin de la Société d'Ethnographie.
Results of Rain and River Observations in N.S. Wales.
Notes upon the History of Floods in the River D.
Notes upon Floods in Lake George.
The Sydney University Calendar for 1887.

Dr. Marloth read his paper, "The Acacias of Southern Africa," stating that the landscape north of the Orange River received its peculiar character mainly from the, in parts, almost exclusive growth of the genus of acacias. This fact induced him to devote some study to the Acacias of Southern Africa. He mentioned that there are two distinct groups of this plant, one of which alone numbered four hundred varieties. The plant is distributed over the continents of Australia, Africa, Asia and America. Twenty-five varieties belong

to South Africa, six of which are found along the eastern coast districts, three in the eastern part of the Transvaal only, one variety reaches the neighbourhood of Cape Town, and the remaining fifteen thrive chiefly in the Kalahari. Excellent specimens of nearly all these varieties were exhibited during the lecture by Dr. Marloth from collections made during his explorations.

The ensuing discussion was carried on by Messrs. Bolus and Péringuey and the Rev. G. W. Stegmann ; the latter stated that the roots of some of the acacias growing in the Oudtshoorn District reached to a depth of 36 feet. He also extended an invitation to botanical members of the Society to visit Oudtshoorn, promising a rich harvest to collectors since, so far as he knew, Oudtshoorn had been neglected by botanists. Prof. MacOwan testified in warm terms to the great value of this most interesting and carefully prepared paper and moved a cordial vote of thanks to Dr. Marloth which was carried by acclamation.

Dr. J. A. Ross then read "Some Notes on Bacteria in Relation to Public Health." He explained that although in many cases Bacteria were fatal to human and animal life it was doubtful whether life would be possible without Bacteria. He strongly advocated the establishment of a laboratory under Government auspices for the purpose of investigating the various scourges which hampered farming so much in this country, and recommended that more study should be given to the subject in future. Dr. Ross exhibited several well-prepared specimens by means of his fine microscope.

An animated discussion was carried on by Messrs. Bolus, Péringuey and Dr. Marloth, and the meeting closed with a unanimous vote of thanks to Dr. Ross for his interesting paper.

Ordinary Monthly Meeting.

WEDNESDAY, NOVEMBER 30, 1887.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

Miss L. A. Robinson was duly elected an Ordinary Member of the Society.

The following donation was announced and the thanks of the Society voted to the donor :

Boletin de la Academia Nacional de Ciencias en Cordoba. Vol. IX.

Mr. Trimen read his notes on "Some Insects Injurious to Vegetation on the Island of Ascension." A collection of several species of these insects, which had been received from the Farm Bailiff at Ascension contributed much towards the illustration of Mr. Trimen's remarks on them and their natural enemies.

The thanks of the meeting were voted to Mr. Trimen.

The President next read Mr. W. B. Tripp's papers on the Rainfall S. Africa. 1st: "Comparisons of Curves of Rainfall with those of Sun-spot Energy 1842 to 1886." 2nd: "Particulars from all Stations from which records of the Rainfall have been obtained for 10 complete years and upwards up to the year 1886."

The President pointed out that Mr. Tripp's giving the distances between the Meteorological Stations will probably convince the public of the necessity of multiplying these stations, particularly between Aliwal North and Pietermaritzburg.

A discussion ensued in which Drs. Ross and Marloth and Prof. Guthrie took part. The latter remarked that it appeared useless to connect rainfall with sun-spot energy.

The unanimous thanks of the meeting were accorded to Mr. Tripp.

Ordinary Monthly Meeting.

WEDNESDAY, JANUARY 25, 1888.

MR. ROLAND TRIMEN, F.R.S., IN THE CHAIR.

Mr. Trimen exhibited on behalf of the captor, Mr. C. W. Morrison, of Estcourt, Natal, a perfect example (received on loan through Col. J. H. Bowker) of the extremely rare, and remarkably beautiful *Lycænide* butterfly, *Aphnæus Hutchinsonii*, Trimen. The species was founded on a single worn example of the male taken at Estcourt by Mr. J. M. Hutchinson in 1886, and described in Trimen's "South African Butterflies," Vol. II., p. 148 (1887). The type specimen (presented to the South African Museum by Mr. Hutchinson) was also exhibited by Mr. Trimen, together with four other South African species of the genus *Aphnæus*. The peculiar beauty of *A. Hutchinsonii* (which is a considerably larger insect than most of its congeners) consists in the numerous large glittering silvery or pearly spots of the underside, relieved by black rings on an olive-greenish ground. The nearest recorded ally of this butterfly is *A. Orcas* (Drury) of Tropical Western Africa.

Mr. D. C. F. Moodie's paper on "The Ethnology of the Zulus" was then read.

Mr. Selous (a visitor) remarked that the accounts of old forts, buildings, &c., given in many books by South African travellers must be received with great caution. He said that stone walls were to be found everywhere in Mashonaland, and he felt sure they had been built by the Mashonas to enclose and protect their kraals. They had pieces let in of a peculiar pattern which was also carved by the Mashonas on their knives.

After some further remarks by Dr. Marloth and Mr. Trimen the thanks of the meeting were voted to Mr. Moodie for his paper.

Ordinary Monthly Meeting.

WEDNESDAY, FEBRUARY 29, 1888.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

The following presents were announced and the thanks of the Society voted to the donors :

Journal and Proceedings of the Royal Society of N.S. Wales, Vol. XX, 1886.

Bulletin de la Société Impériale des Naturalistes de Moscou, 1887, No. 4.

Prodromus of the Zoology of Victoria. Decade XV.

Smithsonian Report, 1885, Part I.

Transactions of the New York Academy of Sciences, Vol. V., Nos. 7 & 8.

Annals of the New York Academy of Sciences, Vol. III, Nos. 11 & 12.

Bulletin of the California Academy of Sciences, Vol. II, Nos. 6 & 7.

Proceedings of the American Philosophical Society, Nos. 124 & 125.

Annual Report of the Department of Mines, N.S. Wales, 1886.

Geology of the Vegetable Creek Tin-Mining Field, New England District, N.S. Wales.

Dr. Marloth exhibited and described a specimen of the so-called Candle-bush.

Mr. Péringuey exhibited a snake commonly called the egg-eater, which was caught at Admiralty House, in the act of swallowing an egg.

Mr. Fisk said that the late Mr. Oakley had one of these snakes in captivity for some time, and he had seen the shell after ejection by the snake: it was a harmless snake, but had the manner and appearance of a venomous one.

Prof. MacOwan called attention to the deaths of three distinguished scientists who had taken considerable interest in S.A. Botany: Dr. Asa Gray, father of American Botany, Dr. Georg Winter of Leipzig, and Dr. Hugo Lojka of Buda-Pesth.

Dr. Asa Gray, the patriarch of American Botany, who had for many years accepted and studied out botanical specimens from this country, died December, 1887.

Dr. Georg Winter of Leipzig, one of the most indefatigable of modern mycologists, who had been engaged upon a new edition of Rabenhorst's Fungi of Europe, took much interest in S. African Fungi, described and figured many of them. He died August 16, 1887, at a comparatively early age.

Dr. Hugo Lojka of Buda-Pesth, Hungaria, a well-known lichenologist, had for several years been engaged in the study of Cape Lichens. His death on September 10, 1887, seemed to have stopped further investigation, but fortunately his work is being carried on by Dr. Stitzenberger of Konstantz, who will probably in the course of the next two years bring out a descriptive catalogue of all known lichens of Africa from the Algerian coast right away to our southern Cape of Good Hope.

The Rev. Professor Foot then read his paper on "Conservation of Energy in its relation to Mind."

In the discussion which ensued Messrs. Fuller, Gill, Péringuey, Guthrie and Beck took part, and the thanks of the meeting were voted to Professor Foot for his paper.

Dr. Gill asked that the next meeting should be held at the Royal Observatory, instead of at the usual place, as he would contribute a paper on the application of Electricity to Astronomy, and the necessary experiments could more easily be performed there.

The President gave a short account of the Comet now visible and of its future path.

Ordinary Monthly Meeting.

WEDNESDAY, MARCH 28, 1888.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

Mr. J. B. Lindley, M.A., LL.B., was balloted for, and duly elected an Ordinary Member of the Society.

Dr. Gill described the various uses to which electricity was put at the Royal Observatory, where the electric light has recently been installed; and the Members and their friends then proceeded to inspect the dynamo and engine with which the electricity is generated, the accumulators where it is stored, and the astronomical instruments to which it has been applied for illuminating purposes, viz.: Transit Circle, 7in. Equatorial, Great Indian Theodolite, Zenith Telescope, Photographic Instrument, and the new Heliometer.

Ordinary Monthly Meeting.

WEDNESDAY, APRIL 25, 1888.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

Professor Guthrie read a paper on "The Theory of the Distribution of Plants."

Dr. Marloth and the President made some remarks upon the paper, to which Prof. Guthrie replied and the meeting closed with a vote of thanks to Prof. Guthrie for his paper.

Ordinary Monthly Meeting.

WEDNESDAY, MAY 30, 1888.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

The following presents were announced and the thanks of the Society voted to the donors:—

Jahrbücher der K. K. Central-Anstalt für Meteorologie und Erdmagnetismus, 1886.

Bulletin de la Société Impériale des Naturalistes de Moscou, 1888, No. 1.

Report of Committee appointed to examine into the Scientific Value of Volapük (American Philosophical Society).

Boletin de la Academia Nacional de Ciencias en Cordoba, Vol. X., Part 1.

Victorian Year Book, 1886-87.

Professor MacOwan proposed that the Society should purchase the back parts of the incomplete series at present in the Society's Library. This proposition was referred to the Council.

Mr. Trimen exhibited a series of specimens (received at the Museum from Mr. Edward Lovett, of Croydon near London) illustrating the still extant but almost extinct manufacture of Gun-flints at Brandon in Suffolk. He called attention to an interesting account of the process, contributed to the Society of Antiquaries of Scotland, and published in the "Proceedings" of that Society for 1887.

In the discussion that ensued Dr. Atherstone, Prof. MacOwan and Dr. Marloth took part.

Dr. Gill drew attention to the "Report of the Committee appointed to examine into the Scientific Value of Volapük," just received from the American Philosophical Society, and suggested that some member should report to the Society on the paper.

Prof. Foot was requested to give a report on the subject.

The Rev. G. H. R. Fisk exhibited a snake, *Dryophidæ*, genus *Langaha*, species *Crista Galli*, a wood snake peculiar to Madagascar.

He also placed on record some measurements of *testudo pardalis*.

Mr. W. E. Fry exhibited diagrams of Isobars 1888, May 21—29, shewing the course of the low pressure during the recent storms.

Ordinary Monthly Meeting.

Wednesday, June 27, 1888.

Mr. W. H. Finlay, M.A., F.R.A.S., President, in the Chair.

The following presents were announced and the thanks of the Society voted to the donors :—

Prodromus of the Zoology of Victoria, Decades I. to XIV.

Uber die Entstehung des Seifengoldes, von E. Cohen.

Goldführende Conglomerate in Süd-Afrika, von E. Cohen.

The Rev. G. H. R. Fisk exhibited a tortoise found near Richmond, genus *homopus*, of which he described the peculiarities—probably new to science.

Mr. T. Stewart read a paper on "The Geology of the Springs in the Cape Peninsula."

The paper was illustrated by several diagrams.

Dr. Atherstone thought the botanical conditions affecting rainfall might with advantage be notified.

Mr. Hutcheons asked about the length of time the rain takes to affect the springs. He had noticed an increase of water in springs just before rainfall, apparently due to low pressure of atmosphere.

Mr. Cairncross stated that the main spring takes about three months before it is affected. He had noticed springs on Breda's estate were increased just before rainfall.

Mr. Stewart replied to the various questions and the meeting closed with a vote of thanks to him for his interesting paper.

Ordinary Monthly Meeting.

WEDNESDAY, JULY 25, 1888.

PROFESSOR MACOWAN, B.A., IN THE CHAIR.

The following presents were announced and the thanks of the Society voted to the donors :—

Journal of the Royal Society of N.S. Wales, Vol. XXI, 1887.

Untersuchung eines Nephelinyenit aüs dem mittleren Transvaal, S.A.

Mr. T. Stewart then read his paper on "The Geology of the Springs in the Cape Peninsula" (read at the last meeting).

In the discussion that ensued, Dr. Marloth, Prof. MacOwan and others took part and Mr. Stewart replied to the various questions asked.

The Rev. Prof. Foot then read a paper on "Volapük."

Dr. Gill, Dr. Marloth, and Dr. Atherstone made some remarks to which Prof. Foot replied, and the meeting then closed with the usual votes of thanks.

Annual General Meeting.

WEDNESDAY, AUGUST 29, 1888.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

The Reports of the Secretary and Treasurer were read and adopted.

The members present proceeded to the election of a President and Council for the ensuing year, with the following result:

President: W. H. FINLAY, M.A., F.R.A.S.
Council: D. GILL, LL.D., F.R.S.
P. MACOWAN, B.A., F.L.S.
F. GUTHRIE, LL.B.
R. TRIMEN, F.R.S., F.L.S., F.Z.S.
HON. J. X. MERRIMAN.
HON. C. A. SMITH, M.A.
R. MARLOTH, M.A., PH.D.
J. H. M. BECK, M.B.
L. PERINGUEY, F.L.S.
H. BOLUS, F.L.S.

Mr. Finlay delivered the Presidential Address on "The Progress of Astronomy during the last 12 years."

A vote of thanks to Mr. Finlay for his very interesting and instructive address was proposed by Dr. Gill and carried unanimously.

REPORT ON THE PROCEEDINGS OF THE SOUTH AFRICAN PHILOSOPHICAL SOCIETY

During the Year Ending July 31, 1888.

1. Since the last Annual General Meeting eleven Ordinary Meetings have been held. The average number of members present has been fourteen, and of visitors eleven, making an average total attendance of twenty-five.

2. At the Ordinary Meetings fifteen papers have been read in the subjects Astronomy, Biology, Botany, Economic Science, Ethnology, Geology, Meteorology, Psychology and Zoology. Notes and shorter communications on a variety of subjects have also been brought before the Society; brief accounts of these will be found in the Notes of Proceedings.

3. Twenty-four presents of books have been received during the year. A list of these will be found in the Proceedings of the Society.

4. During the year seven Ordinary Members have been elected, and seven resigned; the name of one member has been removed for non-payment of subscription. The total number of Ordinary Members is sixty-seven, and of Corresponding Members twenty-three.

5. Vol. IV. part 1 has been printed and distributed to members. Vol. IV. part 2 is now in the press and will be distributed in a few weeks. Authority has also been given for the printing, in England, of a Part of the Transactions containing Mr. Bolus' plates of Cape Orchids, and the accompanying letterpress.

DAVID GILL,
General Secretary.

DR. THE TREASURER in account with the South African Philosophical Society for the Year ended 30th June, 1888. CR.

1887		£	s.	d.	£	s.	d.	1887.		£	s.	d.	£	s.	d.
1 July.	To Balance :								By Printing account ...	45	5	4			
	Bank Balance ...	90	16	9					„ Contribution towards Printing Plates of S.						
	Mortgage Bond...	200	0	0					A. Coleoptera ...	30	0	0			
					290	16	9						75	5	4
	„ Subscriptions ...	112	0	0					„ Clerical Assistance ...		...		32	0	0
	„ Overpayments on same	0	9	6					„ Rent		...		15	0	0
					112	9	6		„ Sundries :						
	„ Interest on Mortgage Bond ...		...		14	0	0		Postage, Railway fares, Cheque Book, &c.	5	6	8			
									G. P. Office, Collecting Postal Draft ...	0	4	6			
													5	11	2
													127	16	6
								1888 30 June.	„ Balance :						
									Mortgage Bond ...	200	0	0			
									Bank Balance ...	89	9	9			
													289	9	9
					417	6	3						417	6	3

Cape Town, 28th August, 1888.

C. ABERCROMBIE SMITH, Hon. Treasurer.

We, the undersigned, Members of the S. African Philosophical Society, hereby certify that we have examined the above Account, compared the Cash payments with the Vouchers and the balance with the Mortgage Bond and Bank Pass Book, and found the same correct.

Cape Town, 28th August, 1888.

JOHN E. B. ROSE,
L. MARQUARD, } Auditors.

ANNUAL ADDRESS TO THE MEMBERS OF THE SOUTH AFRICAN PHILOSOPHICAL SOCIETY,

On the 29th August, 1888,

By the President W. H. Finlay, M.A., F.R.A.S.

THE PROGRESS OF ASTRONOMY DURING THE LAST TEN OR TWELVE YEARS.

I propose to bring before you this evening a review of the Progress of Astronomy during the last 10 or 12 years, a review at least of *some* of the chief points in which our knowledge has been advanced, as an account of all of them would be too long for this address. The theory of Astronomy during this period has not attracted so much of the attention of the younger generation at our Universities, their attention having been diverted to the newer theory of Electricity with its manifold developments ; there is however a goodly number of advances in theoretical Astronomy to be recorded, but I cannot venture on a detailed account of them to-night.

A large amount of work has been done in connection with the shape and dimensions of the earth. Bessel's value of the ellipticity, or ratio of the difference of the axis of the earth's spheroid to the major axis, was $\frac{1}{300}$. It was derived from arcs of the meridian measured in France, Sweden, Russia, India and Peru. From extensions of these arcs and new ones, measured with more refined instruments Col. Clarke has shown that Bessel's value is too small and that $\frac{1}{294}$ is nearer the truth. His result has been confirmed by pendulum observations in India by Col. Herschel, and by the U.S. Coast Survey in America. But it has been made abundantly manifest in these investigations that no one spheroid or ellipsoid will represent the earth's shape with exactitude : and that just as there are departures from uniformity on the earth's surface in the shape of mountains, there are anomalies in astronomical latitudes and longitudes. Notable examples of these have been found in Russia and the island

of Mauritius, and they probably exist to a greater or less amount elsewhere. Obviously then the proper method of procedure is to adopt some standard spheroid or ellipsoid which shall represent as closely as may be the average shape of the earth and to tabulate the deviations from this standard for each particular place. In this way the inconvenience of altering our tables with every additional arc that is measured would be obviated, while every such arc would add its quota to our knowledge of the true shape of the earth. The approximate determination of the latitude of a place is a simple matter, but an accurate determination free from errors in the refraction tables and from instrumental defects is a very different thing. Prof. J. C. Kapteyn, of Groningen, has proposed a method for securing an accurate result which is now being carried out at the Cape Observatory, but the observations are not yet completed. Numerous determinations of difference of longitude by telegraphic signal have been made. Thus Europe and America have been connected by four independent cable operations: S. America and the United States have been connected, and also S. America and Lisbon. During this last operation, which was carried out by officers of the U. S. Navy, an error of $8\frac{1}{2}$ seconds of time (equal to about $2\frac{1}{4}$ miles) was discovered in the accepted longitude of Lisbon. How such an error in the position of a place like Lisbon could have remained undiscovered is inconceivable! Australia and New Zealand have been connected with India: India with Aden, the Cape with Aden, and Aden with Greenwich. Besides these cable operations the Transits of Venus in 1874 and 1882, and several Eclipse expeditions have led to astronomical determinations in out of the way places, so that, although there yet remains plenty of work to be done in connecting intermediate stations, we may say there are few important places on the earth where accurate Greenwich time is not known. With regard to intermediate telegraphic connections I will only mention those carried out in S. Africa, as being of more immediate interest to ourselves. The Cape Observatory has naturally been the standard reference point and with it Durban and Newcastle (Natal), Kokstad, Berlin, Umtata, Aberdeen Road, Montagu Road (now Touws River), and Port Elizabeth have been connected. Five of these determinations were made by Major Morris, R.E., in the course of the Trigonometrical Survey of Natal and the Cape Colony. This survey, undertaken by Natal and the Cape Colony at the instigation of Dr. Gill and under his superintendence, has been carried out by Major Morris with the

most scrupulous accuracy and with an energy that few men could maintain under the difficult circumstances of such a work. It has now been going on for five years: a chain of triangles has been carried from Newcastle through Natal and into the Colony as far as Port Elizabeth, and base lines were measured at Pietermaritzburg and Port Elizabeth. Everything, it must be remembered, in such work depends on the accuracy with which the base (or starting) line is measured, and the exact co-efficients of expansion of the bars used have yet to be determined; but still, with the preliminary co-efficients adopted, the length of the Port Elizabeth base calculated from the Pietermarit burg base carried through the 50 triangles over a distance of 600 miles differs from the measured length by less than two inches and small corrections have yet to be applied which will probably diminish this discordance. It will perhaps give a better idea of the accuracy attained if I put it in this way: to reconcile the two bases exactly it is necessary to correct each of the angles of the triangles by 1-10 of a second of arc, a quantity which we cannot measure with certainty with instruments of the present day.

Truly, S. Africa should be proud of having accomplished such a work and I wish it were more generally appreciated at its true value. When completed it will be an important geodetic measurement, second to none in accuracy, and of lasting benefit to the country in supplying exact reference points for future surveys: we shall then be able to say with truth that S. Africa is the best surveyed colony in the world.

Considerable progress has been made in our knowledge of the tides by Mr. Ferrel, Sir W. Thomson and Prof. Darwin. The height of the tide is regarded as being the algebraic sum of a number of tides due to various theoretical causes: by means of harmonic analysis the constants of these tides are determined from the records at any port, and then Sir W. Thomson's tide-predicter enables us from these constants to predict the tides for any year at the particular station with accuracy. Self-recording tide gauges have for several years been at work at Cape Town, Port Elizabeth and East London, and also self-recording anemometers. They were established at the suggestion of Dr. Gill by the Colonial Government, but most unfortunately funds for the reduction of the records are not forthcoming.

THE MOON.

In the theory of the Moon's motion there has not been much progress, though important memoirs have appeared dealing with special

points. Hansen's lunar tables, published in 1860, were an immense advance on all previous tables: they represented the Moon's motion with great accuracy over the whole range of reliable observation from 1750 to 1864, and they also gave a fair representation of the somewhat vague accounts of total eclipses in ancient times. It was known from Prof. Adams' researches that Hansen's value of the secular acceleration of the Moon's mean motion was nearly double its theoretical value, but a possible and plausible explanation of this discrepancy was found in the action of the tides on the time of revolution of the earth. But from 1865 to the present time the Moon has been gradually running away from the tables more and more every year, so much so that in the year 1887 the errors of the tables amounted to as much as one second of time. Naturally many attempts have been made to discover the cause of this unpleasant disagreement between theory and observation. Mr. Neville, Director of the Durban Observatory, is at present engaged on a revision of the lunar theory. Sir G. Airy has published a Numerical theory, founded on Delaunay's work, which however has not turned out to be all its distinguished author could have wished. Prof. Newcomb, while engaged in preparing accurate places of the Moon for use in the Transit of Venus expedition, detected several small errors in Hansen's work but was obliged, in order to satisfy the observations discussed, to introduce further empirical corrections. The introduction of such corrections really amounts to determining the Moon's mean motion from recent observations only, and there is no reason to hope that they will hold good for more than a few years: in fact evidence is not wanting to shew that they are beginning to fail already. Mr. Stone has brought forward an explanation, which however has not met with acceptance among astronomers: nevertheless, on his hypothesis, Hansen's tables continue to represent the Moon's place as well now as they did formerly. Several other distinguished mathematicians are also at work on the subject, and it is to be hoped their labours will let more light on this difficult theory; but at present the Moon fully deserves its title of "the refractory satellite."

A most valuable series of observations on lunar heat has been made by Prof. Langley by means of the bolometer, an instrument of his own invention. The *rationale* of this instrument is briefly as follows: an excessively thin strip of platinum wire is inserted in two arms of an apparatus which is practically a Wheatstone's bridge and a current of electricity is passed through it; rays from the object whose

heat we wish to determine are allowed to fall on the wire in one of the arms and any increase of temperature acting on the platinum wire retards the flow of electricity ; the balance is disturbed and the effect is immediately shewn on a galvanometer inserted in the circuit. With this instrument he found in 1885 that besides the reflected heat from the Moon's surface there were distinct traces of radiated heat, and he came to the conclusion that the temperature of the surface was about the freezing point. But even this amount of heat implies some atmosphere on the Moon, and the existence of which has always been negatived by other observations.

THE SUN.

The great question of the Sun's distance from the earth is still unsettled, though the uncertainty has been reduced to within narrow limits. First, in the magnitude of the undertaking, came the expedition for observing the Transits of Venus over the Sun's disc in 1874 and 1882. For the former one especially almost every civilized nation in the world equipped parties of observers, who were despatched to all quarters of the globe where the phenomenon could be favourably observed. It was fondly hoped by many that these two transits would settle the question of the Sun's distance beyond doubt or cavil, but this opinion was not shared by those who had studied the reports of the appearances observed in the transits of 1761 and 1769. But still it was expected that with greatly improved instruments and more experience good results would be obtained ; besides it would have been an unpardonable piece of neglect not to observe these transits, which are only visible to one generation out of three. And good results *were* obtained, better I think in the transit of 1882 than in that of 1874 ; but not by any means good enough to settle the question. In fact the method is theoretically a very powerful one, but the optical appearances presented change so gradually and are so perplexing under various conditions of definition that it is almost impossible for observers, widely separated and under different atmospheric conditions, to hit upon exactly the same phase for observation. And here I should like to record my protest against a statement one often meets with in popular astronomical literature ; it is said that the method is worthless because different computers have from the same data deduced such different values for the Sun's parallax as $8''{\cdot}57$ and $8''{\cdot}91$. It is quite true that from the transit of 1769 Encke did deduce the value $8''{\cdot}57$, and on the authority of his discussion the value of

95 millions of miles was given for years and years as the Sun's distance; but in 1868 Stone shewed in the clearest manner possible that Encke had made a complete mistake in interpreting the observations at Otaheite, and that when this mistake was rectified the value given was 8″·91. A somewhat similar mistake was made in the first result published from the British observations of the 1874 transit, and the error was again pointed out by Stone. I venture to say that no astronomer, who has seen the phenomena himself, will question the accuracy of Stone's corrections; and yet these mistakes are constantly trotted out to prove that the method is worthless. When the time comes round again for the next transits in 2004 and 2012 they will no doubt be observed, but the question of the Sun's distance will have been settled long before that. In 1877 a notable attempt to solve the problem was made by Mr. Gill, at the island of Ascension, by the method known as that of "Diurnal parallaxes." The planet Mars at some of its oppositions approaches the earth comparatively closely, and is then considerably displaced with reference to neighbouring stars, according as the observer on the surface of the earth sees it in the morning or evening. With a heliometer stationed on the island of Ascension Mr. Gill determined the amount of this displacement with a very high degree of accuracy and the value of the solar parallax deduced was 8″·78. This value is considerably smaller than what was judged to be the most probable value pointed out by other good methods, but it is certainly entitled to great weight.

The velocity of light in combination with the value of the constant of aberration supplies us with another method of determining the solar parallax. Recent determinations of the velocity of light have been made by Cornu, Michelson and Newcomb. Newcomb's experiments were by far the most complete and elaborate, and his result for the velocity of light *at the earth's surface* is probably not in error as much as twenty miles a second. A new value of the constant of aberration was deduced about the same time, 1882, by Nyrén of the Poulkova Observatory; and this value combined with Newcomb's light velocity gives a parallax of 8″·79, which is strikingly confirmatory of Mr. Gill's result. There is one element of doubt, however, in this method; can we assume that light travels with exactly the same speed at the earth's surface as it does in interstellar space? We cannot tell. In 1872 Galle, of Berlin, proposed that some of the minor planets which come moderately near the earth should be pressed into the service, and an attempt was made in 1873 with the planet

Flora. As proposed by him the method required the co-operation of observatories in the northern and southern hemispheres, and owing to various unfavourable circumstances the observations in the southern observatories were not sufficiently numerous and good. Another attempt was made in 1882 on the planets Victoria and Sappho ; no result has yet been deduced from these observations, but they will certainly give a very fair result. A grand attack on the problem will be made this year and next with the exquisite heliometer which has recently been erected at the Cape, both independently by the method of diurnal parallaxes and also in conjunction with similar instruments in America and Germany, and results of the highest degree of accuracy may be confidently expected. It is perfectly certain that the minor planet method will give the most reliable determination at the present time, but it is almost equally certain that the final value, to be adopted many years hence, will be deduced from the perturbation of the planets Venus and Mars under the action of the earth as pointed out and already approximately done by Leverrier ; for this method is one that ever increases in accuracy with the lapse of time.

A very important series of experiments has been carried out by Prof. Langley on the energy of the separate rays constituting the lower part of the solar spectrum. In previous investigations the spectrum had been formed by refraction through a prism, as no instrument existed of sufficient delicacy to measure the heat of the diffraction spectrum. But in the prismatic spectrum the rays are much more crowded together at the red end than at the blue, and the effect of previous investigations was consequently to indicate a maximum of heat in the infra-red portion of the spectrum. Hence arose the three curves of solar energy which we are accustomed to find in the text-books, viz., the actinic, light and heat curves, with maximum value at different parts of the spectrum. But in the diffraction spectrum we have a normal arrangement of the rays side by side without any crowding, and Prof. Langley by means of his bolometer was able to measure the heating effect of each portion in detail. He found that the point of maximum heat practically coincided with that of maximum light, *i.e.*, near the sodium lines in the yellow. We are thus led to the conclusion that there is no difference of quality in the rays themselves, but that the various effects produced by them depend on the various capabilities which the bodies they impinge on possess of turning to account the longer or shorter vibrations.

From simultaneous observations at the summit and foot of Mt.

Whitney, Langley was also enabled to calculate the effect of the earth's atmosphere in absorbing the different portions of the spectrum, and he found that the blue end suffered far more than the red. The solar atmosphere has been shown by Vogel and Langley to have a similar effect in stopping the rays of shorter vibration, so that our view of the Sun is as if two plates of red glass were interposed between us, the removal of which might leave the Sun of a bluish colour.

Photographs of the Sun's disc are taken on every fine day at Greenwich, Mauritius, and at Dehra Dûn in India ; so that it rarely happens that there are more than two or three days in the year on which we have not a record of the state of the disc and of the size and shape of the spots on it.

The study of the chromosphere and the solar prominences has been carried on by many observers, among whom the late Director of the Rome Observatory, Tacchini, deserves particular mention. It cannot be said that any striking result has been reached in connection with them, but a sound foundation is being laid for future investigation.

Total Eclipses of the Sun have been regularly and systematically observed, expeditions being despatched to any part of *terra firma* where they may be visible, as *at present* it is only on these occasions that we can see that wonderful solar appendage—the corona. The appearances presented by the corona are very different in different eclipses, and there is a large amount of evidence to show that it is most developed at times of sun-spot minima.

The Eclipse of 1878 was particularly well observed in North America, in one case at an altitude of 14,100 feet above the sea-level, and the great advantage of observing at high altitudes was shown by the fact that the corona was visible for 4^m after totality.

The brighter part of the corona was reduced to a narrow ring of light round the Sun, but remarkable fainter streamers were seen stretching away right and left of the Sun to a distance of ten millions of miles. The axis of the longest ray coincided with the ecliptic. The idea of a connection between these streamers and the zodiacal light immediately suggested itself, and in fact we should expect the zodiacal light (which is certainly a solar appendage) to present much the same appearances at its lower parts as were seen in this eclipse. I may mention that a very similar appearance was seen by Stone in Namaqualand in 1874, but the rays were not visible so far from the Sun's disc. Next in importance was the eclipse of May 17, 1882, the chief

observations of which were made in Egypt. This was a year when sun-spots were numerous and the appearance of the Corona fully supported the connection with them previously enunciated. No zodiacal streamers were seen, and the general aspect was very similar to that of the eclipse in India eleven years earlier. The eclipse of 1883 was observable only from a small island in the South Pacific Ocean but a considerable party of astronomers flocked there to see it ; the Corona was similar to that seen in 1882. Some important results were obtained, and one very suggestive one by Tacchini. He saw in the spectrum of the Corona on a feeble continuous background two of the bright hydro-carbon bands which are found in the spectra of comets ; the observation however requires confirmation. So far as we can say at present the Corona is certainly not a solar atmosphere inasmuch as its pressure does not increase as the sun's disc is approached ; it may be defined as gaseous matter constantly flowing from and towards the sun or perhaps circulating round it, under the action of a repulsive electrical force and of gravity. It is chiefly composed of hydrogen and the unknown substance giving the green ray 1474, and partly of solid or liquid particles shining with reflected and inherent light, but it must be of inconceivable tenuity since comets are able to pass through it without suffering any alteration of velocity.

The fact that we have only some four or five minutes a year at the most for the study of these phenomena makes it highly desirable that some means of observing them at any time should be discovered. This has already been done in the case of the prominences, which are now mapped daily, and an attempt has been made with the Corona. From the photographs taken in 1878 Dr. Huggins observed that the light in the spectrum is largely confined to violet rays, and he hoped to find by excluding all but these rays that the glare produced by the atmosphere plus the coronal light might be sufficiently greater than the glare alone to render the shape of the Corona visible. His first experiments were made in 1882 and gave a fair promise of success ; the impressions on his plates strongly resembled the Corona in general features and in details ; but the haze or dust that was present in the atmosphere after the volcanic explosion in the Sunda Straits in 1883, August, put an end for a time to work in this direction. Further experiments were made by Mr. C. Ray Woods on the Riffel and at the Cape in the following years, but with no better success. The eclipse of 1886 was anxiously looked forward to as a crucial

test ; if the method was a sound one it ought to be possible to obtain a picture during the partial phase of the eclipse shewing the moon's contour against the background of the coronal light. No trace of the Moon however was discernible in the photographs taken at the Cape and Grenada ; and for the present at least the method is not practicable. When the effects of the Krakatoa eruption have entirely passed away, it will without doubt be tried again at some elevated station where the coronal light will not suffer so much absorption by the earth's atmosphere.

Passing now to the other members of the solar system, we may say with certainty that there is no intra-mercurial planet as large as 300 miles in diameter, though it is very probable that there are intra-mercurial asteroids. During the eclipse of 1878 Swift and Watson claimed to have seen two objects where no stars existed and it was generally thought at first that the missing Vulcan had been found ; but on examining their observations it was seen that they could not possibly be brought into harmony, and it is almost certain that they both observed the same two stars in Cancer, the hurry and excitement of the moment leading to errors in the record of their position. An exhaustive search for any such planets was made during the eclipse of 1883 with negative results, and we can only come to the conclusion that there is no such object of any considerable size.

Our knowledge of Mercury and Venus has not been greatly increased during the last ten years, but an interesting and successful attempt has recently been made to clear up the mystery of the satellite of Venus which was seen on several occasions in the 17th and 18th centuries. It has usually been supposed that the object seen was a false image or "ghost" arising from some internal reflection in the telescope, but this explanation has been felt to be weak, inasmuch as the supposed satellite was seen on several of the occasions by experienced astronomers who would be fully aware of the danger of deception from such a cause. Last year Dr. Stroobant made a careful examination of the positions given for the satellite and found that the majority of them coincided sufficiently closely with the places of stars of about the 5th magnitude to make it reasonably certain that the objects seen were these stars ; and he satisfied himself by actual observation that stars of this magnitude could be seen under favourable circumstances by daytime. Thus another astronomical ghost has been laid to rest.

The planet Mars always comes in for a large amount of attention,

chiefly because more of the details of its surface are visible than is the case with the others. This is due partly to its proximity to the earth, but chiefly to the tenuity of its atmosphere. It must be rare to admit of these details being seen and yet judging from the behaviour of what are supposod to be polar snows or ice, the climate seems to be unexpectedly mild. How these apparently contradictory facts are to be reconciled I do not know. In 1877 August a memorable discovery was made by Prof. Asaph Hall with the 26-inch refractor at Washington, viz., that Mars was the possessor of two small moons. And very interesting little bodies these are:—assuming that they reflect the same proportion of the Sun's light that Mars does, they are about 6 and 7 miles respectively in diameter, the inner one being the larger. This inner one revolves round its primary in $7^h.\ 29^m.$ at a distance from its surface of only 3,760 miles, and we are here presented with the unique case of a satellite completing its revolution in less than its primary's day and coming to the meridian twice or even three times in that day. At this same opposition of 1877 Schiaparelli had observed that what had always been called the continents of Mars were really a collection of islands connected by long markings of about 60 miles in breadth which have been called "canals." These were distinctly visible again from 1881 December to 1882 February, but in many cases were seen to be double, *i.e.* another canal was found parallel to the original one; and this duplication has been verified in 1886 and the present year. Travelling farther from the Sun we come to the group of asteroids which have now become so numerous that they require the whole attention of a separate department at Berlin to follow their movements and guard against their being lost. The last discovered was number 278 I believe. Naturally the ones recently discovered are on the average fainter than the earlier ones; they average about 11 or 12 magnitude and are probably not more than 15 miles in diameter.

The chief modern discovery about Jupiter is that he is in a stage midway between the Sun and the Earth; he has not cooled down sufficiently to possess a solid crust, and his vast stores of internal heat give rise to agitations which are shewn by the changes observed in his disc. This discovery however does not belong to the period we are considering. In 1878 Pritchett, Director of the Morrison Observatory (U.S.), observed an enormous rosy cloud near the southern equatorial band; and in 1879 and the two following years the Great Red Spot, as it was called, attracted universal attention. Its colour had deepened

to a full brick-red, and the effect was heightened by the presence of an unusually brilliant white equatorial spot. The two spots completed a revolution in different times, the white one taking about $5\frac{1}{2}$ minutes longer than the other; and neither preserved a uniform period throughout. In May 1883 the red spot had become almost invisible, but in December it began to recover and in 1886 presented much the same appearance as in 1882. The only conclusions as to it nature that we can draw from the numerous observations are merely negative ones; it was not an eruption from a Jovian volcano; it was not attached to the surface of the planet; it was not a portion of a glowing disc seen through a rent in the enveloping vapours; and it was not self-luminous.

Observations of the eclipses of Jupiter's satellites led Römer to the discovery that light does not travel instantaneously, and the method was employed by Glasenapp in 1874 to obtain a new determination of the velocity of light. But the ordinary observation of the time of disappearance or reappearance of a satellite is an unsatisfactory one, and a much better plan has been proposed and is being carried out by Pickering. By means of a photometer he determines the instant when the satellite has lost half its light, *i.e.* when it is half submerged in Jupiter's shadow and finds he can fix this time with great accuracy. These observations when carried out for 12 years will furnish a very reliable value of the light-equation and of the solar parallax.

Saturn is probably in a still earlier stage of development than Jupiter; his specific gravity is so low that he would float in water and as the density increases towards the centre it follows that the surface cannot be solid or liquid; hence we must regard a large proportion of his globe as consisting of heated vapours, kept in constant agitation by the process of cooling. An interesting question suggests itself as to whether the wonderful ring has always maintained the same dimensions and the same distance from the planet. Otto Struve made in 1851 a most careful determination of the dimensions of the system and repeated them under very similar circumstances in 1882. There appeared to be a slight extension in width of the rings both inward and outward, but so slight as hardly to be separated from the possible errors of observation. Some change there certainly has been, for in 1657 Huyghens described the interval between the ring and the planet as somewhat greater than the width of the ring, while at the present time this interval is only about two-thirds the width of the rings.

A magnificent drawing of Saturn and his rings has been made by Trouvelot by the aid of the 26-inch refractor at Washington; and I am fortunate enough to be able to show you a copy of it.

The rotation of the planet Uranus has recently presented a most puzzling problem for the consideration of astronomers. As is well known, the satellites revolve in a plane nearly at right angles to the ecliptic and it might naturally be inferred that the axis about which the planet rotates is situated nearly in the plane of the ecliptic. This inference is supported too by observations of a slight bulging of the disc in the plane of the satellites. But in 1884, from January to June, two grayish parallel markings were distinctly seen at Paris and Nice; and these markings which must be assumed to lie in planes parallel to the planet's equator were inclined at an angle of 40° to the plane of the satellites' motion. This fact was also confirmed by observations of a bright spot which indicated a time of revolution of about ten hours. Does Uranus, then, at the present time rotate in a plane differing widely from the plane of rotation at the far distant time when the satellites were formed? No solution of this difficulty has yet been proposed.

Whether a planet will ever be found beyond Neptune it is of course impossible to say, but it is not at all unlikely that some day one will be found in the course of the search for minor planets. Two attempts have been made to fix the position of such a body from theoretical consideration, one by Forbes in 1880 and the other by Todd about the same time. Prof. Forbes relied on the fact that comets of short period group themselves into families in such a way that when at their furthest distance from the sun they are near the orbit of some one of the major planets; he found two considerable groups of comets which reach their aphelia at distances of 100 and 300 times the earth's distance, and he maintained that there was an unseen planet revolving at each of these distances. Prof. Todd followed the method which in the hands of Adams and Leverrier had led to the discovery of Neptune. The perturbations of Neptune have not had time yet to develope sufficiently, but there are certain anomalies in the motion of Uranus or "residual errors," as they are called, not accounted for by theory on which he based his investigation. It is remarkable that both he and Prof. Forbes assigned places for the hypothetical planet which agreed well with one another.

I must not pass away from the planets without briefly referring to the remarkable investigations of Prof. G. H. Darwin on the subject

of Planetary Evolution. By the most refined mathematical analysis he calculates the effect produced on the motion of two rotating viscous bodies by the "bodily" tides each would produce in the other. The primary effect of a tide raised on such a body is to diminish its velocity of rotation; the tide acts as a brake since the interior portions are revolving as it were in a collar, and gradually the body thus acted on tends to turn always the same face to the other. In the case of the moon this has already come to pass. And further the tidal wave produced on the earth by the moon reacts on the moon and pulls her forward; now a forward pull of this kind means an enlargement of the moon's orbit so that she is gradually receding from the earth. Following this line of reasoning we see that in ancient times the moon must have been very much closer to the earth than at present, and indeed Prof. Darwin traces her history back to a time when she was revolving close to the earth in a period of between two and three hours. In 1881 he published an investigation, following the same principles, into the early history of the whole solar system; and he found that the earth and moon formed a singular exception to the other members of the system, owing chiefly to the moon being much larger relatively to its primary than the other satellites and having in consequence a very much greater tide-producing power. He was led to the conclusion that the satellites of the other planets had never revolved in much smaller orbits than they do at present. Solar tidal friction however played an important part in determining whether a planet was to have moons or not, if we accept the view that satellites were produced by the disruption of a too-rapidly rotating primary, as by checking the velocity of rotation it checked the tendency to disruption. Hence we see that Mercury and Venus, where the solar influence is strong, were unable to rotate with sufficient speed to produce satellites and the earth narrowly escaped the same fate; while the outer planets, where the influence of the brake is weak, are well provided with moons.

COMETS.

No great stride has been made in our knowledge of these bodies in spite of the large number of brilliant ones we have been favoured with, but several advances of considerable importance have been secured. There seems to be little doubt that the heads of comets are aggregations of small particles, the interstices being filled with gaseous vapours, and that the tails are due to electrical repulsion.

between the sun and these vapours. Bredichin's division of the tails into three types corresponding to hydrogen, hydro-carbons, and metallic vapour respectively has been amply verified (at least as regards the two first). But perhaps the most important fact which has been brought to light is that there undoubtedly exist families of comets following almost exactly the same path in space. Formerly identity of path was considered conclusive evidence of identity in the objects, but we can no longer say this. Tebbutt's comet of 1881 was moving in almost exactly the same path as the comet of 1807 for which Bessel in a classical discussion had deduced a period of 1,539 years. Fortunately Tebbutt's comet was visible for a long time and the observations of it proved that it was moving in an orbit of about 3,000 years period, so that it was not a return of the 1807 one. Again the Southern comet of 1880 followed a path almost identical with that of the great 1843 comet which was visible in broad daylight near the sun ; and the great comet of 1882 and the first of 1887 were also moving in this same orbit—yet they are all distinct bodies though undoubtedly members of the same family. It must be remembered that the comets in this group pass close to the sun's surface and right through the corona ; and though observation shewed that the 1882 comet had not suffered the slightest retardation in doing so, (thus giving the final blow to Encke's resisting medium), yet several faint nebulous masses were observed after perihelion travelling along with the comet and at some little distance from it, which had evidently been torn away from the main body. Thus it is not difficult to imagine that the comets of '43, '80 and '87 were separated from the original comet at some previous passage through perihelion ; and indeed it seems to me that we may go further and conjecture that the '43 comet was separated from the parent body (the remains of which I take to be the '82 one) at an earlier approach to the sun than the '80 and '87 comets, partly on account of its much greater brilliancy and partly because its tail belongs to Bredichin's type I, corresponding to hydrogen, the lightest known element, while that of the '80 comet belongs to type II, the hydro-carbons. An interesting and instructive observation, unique in the history of astronomy, was made by two observers at the Cape in 1882 : the "Great September Comet" was followed in the telescope till it reached the sun's limb, when it disappeared completely though it was passing in front of the sun's disc. Shortly before the transit it shewed as a brilliant circular disc 4″ in diameter, but the disappearance proved

that it was not a solid globe of this size but only an aggregation of small bodies, which were sufficiently small and sufficiently far apart not to be visible on the sun. This comet and the one discovered by Wells earlier in the year revealed a new fact in the chemical constitution of these bodies; when they were near the sun the usual hydro-carbon bands seen in the spectroscope were entirely replaced by the bright sodium lines, and these again disappeared and gave way to hydro-carbon bands as the comets receded from perihelion. I have already mentioned that there is a group of comets connected in some way apparently with the planet Neptune and which have a period of about 70 years, one of which is Halley's famous comet. A second was discovered in 1812 by Pons, and a third in 1815 by Olbers; these have both presented themselves to view again, the former in 1883 the latter in 1887; the other members of the group are not yet due. The connection between meteor-swarms and comets is now well established, viz. that these swarms result from the disintegration of a comet; and each return to perihelion of what was once Biela's comet gives us a brilliant shower of these small bodies. The last was in November 1885.

STARS.

We are at least beginning to feel our way, with no uncertain steps, to the determination of the distances of the stars from our system. In 1884 Gill and Elkin published the results of their heliometer observations of the parallax of Southern Stars—a classical investigation far surpassing all previous attempts in accuracy and extent. It is a pleasing fact for Cape astronomers that they confirmed the substantial accuracy of Henderson's value for the parallax of α Centauri, deduced 50 years ago from his own observations with the Cape mural circle. This star still remains, with our present knowledge, the nearest star to us; while β Centauri refuses to yield any sensible parallax. Asaph Hall and Ball have also contributed very valuable work on this question; and the new Cape heliometer is now actively at work on it. Several important catalogues of star-places have been published in the last few years, notably Stone's Cape Catalogue of 12,441 stars, and Gould's Cordoba Catalogue of 32,448 stars. Each of these gigantic pieces of work was carried through in a few years, Stone's in 10 years, Gould's in 15. Up to the year 1870 our knowledge of the accurate positions of stars in the Southern

Hemisphere was of a very meagre description; but now, thanks to the labours of these two astronomers and their assistants, we can safely say that the southern half of the sky is better surveyed than the northern; and it will remain so till the publication of the catalogue of stars to the 9th magnitude now being made under the direction of the Ast. Gesellschaft and by the co-operation of fourteen observatories in Europe and America. In 1860 Dr. C. H. F. Peters commenced the enormous task of forming maps giving the positions of all stars down to the 14th magnitude situated within 30° on each side of the ecliptic. Part of the work was finished in 1882 and will prove invaluable in the search for minor planets; but the photographic survey of the sky, which was resolved on last year and which will shortly be commenced, will replace all similar work in the future. Such a survey, including all stars to the 9th magnitude, is now approaching completion at the Cape Observatory, and the work of measuring the photographs and cataloguing the stars is being done by Prof. Kapteyn of Groningen. Variations in the brightness of stars have always attracted much attention and there are many striking instances of such changes, *e.g.*, the wonderful η Argûs which 50 years ago blazed out till it was the brightest star in the southern skies, while now it is below the limit of visibility to the unaided eye. Extensive catalogues have recently been made giving accurate determinations of the magnitudes of the stars, by Gould for the Southern Hemisphere and by Pritchard and Pickering for the Northern. Gould's estimates were made with the naked eye or by the help of an opera-glass; Pritchard's by means of the "wedge photometer," a wedge of neutral-tinted glass made to slide in front of the eyepiece of the telescope till the point of extinction of the star's light is reached; Pickering's by a polarising apparatus used in such a way as to reduce the light of the stars to equality. Successful photographs of stellar spectra were obtained by Huggins in 1879 and by Cornu in 1886, and this work is now being carried on at the Harvard Observatory on an extensive scale. A large prism placed in front of the object-glass gives at once the spectra of all the stars in the field of view of the telescope; and in January 1886, with an exposure of 34 minutes a photograph was obtained shewing the spectra of 40 stars in the Pleiades, which nearly all belonged to the same type.

Determinations of the velocities of stars in the line of light have been regularly kept up at Greenwich and by Vogel, but the excessive difficulty of the observation has prevented the results hitherto obtained

from being as concordant as could be wished. The observation consists in determining the displacement (due to motion to or from the earth) of a line in the star's spectrum as compared with the same line given by an artificial light; but the smallness of the displacement and the unsteadiness of the stellar line caused by the earth's atmosphere almost make a satisfactory observation hopeless. Here again photography promises to come to our assistance, the sensitive plate does what the eye cannot do, it shews the *accumulated* effect of the various positions of the unsteady line by a space the centre of which corresponds to the mean position of the line. There is very little doubt that this method, which was suggested and has already been successfully tried by Vogel, will entirely supersede the older one of eye observations.

On the many other applications of photography to astronomy I shall not dwell, as it is only a short time ago that we had an exhaustive account of them from Dr. Gill; but it is abundantly evident that the co-operation of electricity and photography have completely revolutionised the older astronomy and opened up new and extensive fields of research.

Many new observatories have sprung into existence in the last 12 years, one I am glad to say in South Africa, viz., the Natal Observatory at Durban. The latest and greatest is the Lick Observatory in California; it is situated on Mount Hamilton at an elevation of 4,200 feet above the sea, it possesses the largest refracting telescope in the world and an excellent climate, and with a staff of experienced observers it has a great future before it.

There are numbers of minor points in which substantial progress has been made, but an account of them all would fill a volume. I have endeavoured to lay before you the more important ones, and I hope I have succeeded in giving you some idea of the problems that have been solved in the last few years and of the unceasing activity and earnestness of the devotees of the noblest of sciences.

MINUTES OF PROCEEDINGS.

Ordinary Monthly Meeting.

WEDNESDAY, AUGUST 29, 1888.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

Mr. W. C. Meredith, M.A., and Mr. J. Martin were elected Ordinary Members of the Society.

Mr. Trimen read some notes on the teeth of a whale, *Mesoplodon Layardii* (Gray), of very peculiar form, and exhibited specimens.

Mr. Woods exhibited some photographs of lightning flashes taken during the recent thunderstorm, and read some notes thereon.

After some remarks by Messrs. Finlay, Marloth and MacOwan the meeting closed with a vote of thanks to Messrs. Trimen and Woods.

Ordinary Monthly Meeting.

WEDNESDAY, SEPTEMBER 26, 1888.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

Mr. D. C. Andrew and the Rev. D. P. Faure were elected Ordinary Members of the Society.

Mr. C. Ray Woods exhibited some photographs of lightning flashes taken during the recent storm.

Messrs. Fisk, Finlay and Péringuey took part in a short discussion.

Ordinary Monthly Meeting.

WEDNESDAY, OCTOBER 31, 1888.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

Prof. MacOwan exhibited a specimen of *Ixianthes retzioides*, Benth., found by Eklon, Poppe and R. Templéman. This specimen with four or five others was found near Waterfall Farm, Tulbagh.

The Rev. G. H. R. Fisk read a note about the tortoise exhibited by him at the meeting held on June 27 last. It had been ascertained that the tortoise referred to was not a new species but merely an anomalous species of *Testudo semiserrata.*

The Rev. Dr. Kolbe then read Mr. Hammond Tooke's paper on "The Star-lore of the S.A. Natives," to which was appended some notes by Dr. Kolbe senior.

The Rev. Dr. Kolbe stated that the Damaras have a word for Comets, viz., "the long thing which makes people enquire"; the milky way they call "rising steam." They once knew something about meteors. The Ven. Archdeacon Lightfoot said that the coloured races in Cape Town have no traditions about the stars.

The Rev. G. H. R. Fisk said that every endeavour should be made to secure photographs, &c., of old Bushman paintings and specimens of remains.

The Rev. G. Stegmann had tried to get specimens from caves in Oudtshoorn. In several cases, at a depth of four or five feet he found ivory arrow-heads stained blue, and a bar of horn, bone needles, pots, teeth of animals, a pipe with three bowls running into one stem, made of clay.

A vote of thanks to Mr. Tooke and Dr. Kolbe closed the proceedings.

Ordinary Monthly Meeting.

Wednesday, November 28, 1888.

The Rev. Professor Foot in the Chair.

The undermentioned gentlemen were duly elected Ordinary Members of the Society:

Mr. Alexander Mair, Mr. W. J. Murphy, Mr. D. C. F. Moodie.

The Rev. G. H. R. Fisk exhibited a snake and a tortoise.

The Secretary read Mr. Hammersley-Heenan's paper "On the action of Teredo Navalis, &c., on the piles of Port Elizabeth Jetty."

Mr. D. C. F. Moodie read a paper on "The Northern Gold Fields of South Africa," the completion of which was postponed till next meeting.

Ordinary Monthly Meeting.

WEDNESDAY, FEBRUARY 27, 1889.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

The undermentioned presents were announced and the thanks of the Society voted to the donors :

Geological and Natural History Survey of Minnesota, 15th Annual Report, 1886.

Geological History Survey of Minnesota, Bulletins Nos. 2, 3, 4.

Bulletin of the California Academy of Sciences, Vol. 2, No. 8.

Proceedings of the American Philosophical Society, Nos. 126, 127.

Smithsonian Report, 1885, Part 2.

Mr. Bolus exhibited an orchid found (Jan. 2) on Constantia Berg by Mr. Bodkin. It had been found about fifty years ago near Caledon, and two years ago near Stellenbosch—now, for the first time, in the Cape Peninsula.

The Rev. G. H. R. Fisk exhibited specimens of the complete series *Homopus Areolatus*, *Homopus Femoralis* and *Homopus Signatus* —the latter exhibited here for the first time alive. The specimen is one of three captured in Clanwilliam. The other two have been sent to the Zoological Society of London.

The Rev. G. H. R. Fisk further exhibited a totally black shell of the tortoise, *Chersina Ungulata* or Angulated Tortoise, found at Verkeerde Vley near Touws River. The living animal was also totally black.

Mr. Trimen read a letter from Mr. Geo. Romanes requesting that some members of the Society would take up experiments on the bite of scorpions. The question to be decided was whether the poison glands are able to absorb the poison of bites inflicted by other scorpions, that is to say, would scorpions if deprived of their poison glands survive the bites of others.

Prof. Guthrie brought forward a table for calculating heights from barometric readings. Assuming the height of Kimberley as known, he deduced a factor by the means of which the known and calculated heights of Aliwal North, Cradock and Graham's Town were shown to agree very closely.

Mr. Finlay, Dr. Gill, Mr. Fourçade (a visitor) and Mr. Howard (a visitor) took part in the short discussion that ensued.

The President, in Mr. Moodie's absence, proceeded with the reading of Mr. Moodie's paper on the Northern Gold Fields, and a vote of thanks to Messrs. Guthrie and Moodie closed the proceedings.

Ordinary Monthly Meeting.

Wednesday, March 27, 1889.

Mr. W. H. Finlay, M.A., F.R.A.S., President, in the Chair.

Messrs. G. P. Moodie and F. S. Lewis, M.A., were duly elected Ordinary Members of the Society.

Mr. R. Trimen exhibited specimens of the "Silver Moth" (*Leto Venus*), from the Knysna, district of the Cape Colony, where they had been collected by Miss Newdigate, and by the late Mr. W. K. Newdigate, near Forest Hall, Plettenberg Bay.

He called attention to the great size and beauty of this splendid insect, features the more remarkable because the moth belonged to a family (*Hepialidæ*) of which the great majority presented small stature and very dull colouring. Specimens of three South African species of *Hepialidæ* were exhibited in illustration of the ordinary members of the family.

Another point of interest was the very limited district which the Silver Moth inhabits. No examples are recorded to have occurred in the Eastern Districts of the Colony, Kaffraria, or Natal; nor is it known from the interior or from any part of Tropical Africa. Miss Newdigate, who has had a long and intimate acquaintance with the insect, states that the Western limit of its range known to her is Oakhurst, between George and Knysna and the Eastern limit Storm River. The Zitzikamma forest tract may thus be said to constitute the only *habitat* of *Leto Venus*, though this is rendered difficult to understand by the fact that the caterpillar's only known food is the wood of the Keurboom (*Virgilia Capensis*) a familiar leguminous tree, with sweet scented pink-and-white flowers which has a wide distribution in the South of the Colony.

The moth is only found from the middle of January to the middle of March, and between the hours of four and six p.m.

The moth was figured in 1782 by the well-known Dutch author, Cramer, on pl. 286 of his "Uitlandische Kapellen (Papillons Exotiques)." (Specimen probably collected by J. A. Ange, botanical collector on an expedition in Governor Tulbagh's time (latter died in 1771), who is known to have collected many of the insects then first sent to Europe.) Cramer is quite enthusiastic on the splendour of its silver markings ; but he was not contented with its extremely short antennæ (a characteristic feature of the family) and therefore made them in his figure of (what he judges to be) a suitable length ! A tolerable wood cut of the moth is given on pl. 8 of Vol. II. of the "Papillons" of Chenu's "Encyclopédie d' Histoire Naturelle" (1857), but no good figure of this grand moth has yet been published.

Mr. Bolus suggested that in all probability its *habitat* was determined by the meteorological conditions of the Knysna.

Mr. Péringuey exhibited some beetles from the neighbourhood of Walfisch Bay—a set of three which he had only received a few days previously from Mr. Carrington Wilmer had enabled him to settle the identity of *Eurymorpha Cyanipes* with *Eurymorpha Mouffleti.* He also exhibited a specimen of the extremely rare *Mantichora Ludovici,* which seems to be represented in European collections by one example only. He also exhibited a small tiger beetle, *Ciandela nilotica,* found (for the first time in South Africa) by Mr. Carrington Wilmer : this insect has been found from Cairo to the Blue Nile, in Senegambia, and now in South Africa.

The President then read Mr. H. C. Schunke's paper "On a Method of preparing Silhouettes for the purposes of Anthropology."

In the discussion that ensued Messrs. Péringuey, Bolus, MacOwan, Finlay and Fisk took part and the proceedings closed with the usual vote of thanks.

Ordinary Monthly Meeting.

Wednesday, April 24, 1889.

Professor Guthrie, LL.B., in the Chair.

The undermentioned presents were announced, and the thanks of the Society voted to the donors :

Victorian Year Book, 1887–8.

Catalogue of Canadian Plants, Part 4, Endogens.

Transactions and Proceedings and Report of the Royal Society of South Australia, Vol. X.

Mr. Bolus exhibited a specimen of *Vaccinium exul*, a new species of that plant, found at the Devil's Kantoor on the Drakensberg, 30 miles from Barberton, 5,400 feet above sea level. No plant of that genus had ever been found previously so far south in the Southern Hemisphere. It was noticed that the fringe of mountains which ran round a large portion of Africa was the pathway of connection of plants. It was always on the mountain ranges that Northern Hemisphere plants were found in the Southern Hemisphere and *vice versa*. When this migration of plants took place had never yet been determined.

Prof. Guthrie stated that the discovery of this plant was especially interesting as a new fact in the geographical distribution of plants from which the history of the world could be written.

Dr. Gill then gave a most interesting account of recent results of the Geodetic Survey of South Africa, which had now been completed from Natal to Port Elizabeth. He first gave a short history of various earlier attempts at survey in the Colony, and then proceeded to describe the method of measuring the base lines, the instruments employed, and the accuracy obtained. He stated that the Transvaal had voted £10,000 for a similar survey and it had been proposed that such a survey should be carried out in Bechuanaland, so that soon South Africa would have a survey that would be a credit to the country.

Mr. Marquard, Prof. Guthrie, and Mr. Justice Buchanan made some remarks to which Dr. Gill replied and the meeting closed with the usual vote of thanks.

Ordinary Monthly Meeting.

Wednesday, May 29, 1889.

Mr. W. H. Finlay, M.A., F.R.A.S., President, in the Chair.

The African traveller and hunter, Mr. F. C. Selous, gave a most interesting account of a journey made by himself beyond the Zambesi to the country of the Mashukulumbwe.

Ordinary Monthly Meeting.

WEDNESDAY, JULY 31, 1889.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

The undermentioned presents were announced and the thanks of the Society voted to the donors :

Bulletin de la Société Impériale des Naturalistes de Moscou, 1888, No. 3.

The American Anthropologist, Vol. I. pts. 2, 3, 4, Vol. II. pt. 1.

Journal of the Cincinnati Society of Natural History, Vol. XI.—4.

Proceedings of the American Philosophical Society, No. 128.

A Grammar of the Kwagiutl Language (Transactions of R.S. of Canada, Vol. VI., Section II.)

Feuille des Jeunes Naturalistes, No. 225.

Mikrographie der Mitteldermdrüse (Leber) der Mollusken.

Versteinerungen der Cambrischer Schichtensystems.

The Rev. G. H. R. Fisk exhibited two specimens of manganese markings on sandstone.

The President then read Mr. Carrington Wilmer's paper on "The Relation of the Sand-dune Formation on the South-west Coast of Africa to the Local Wind Currents."

Mr. Péringuey said this large belt of sand extended down the coast to Blaauwberg, and that from Cape Blanco to the Congo there was a similar stretch of sand. He did not believe that these sand-dunes were of recent origin for the following reason : There was one genus of Coleoptera common to both the East and West Coasts of Africa, and the one found on the West Coast possessed a special formation of the legs for moving quickly over sand. This formation must have taken ages to bring about.

Messrs. Fisk, Gill, Marloth, and Prof. Seeley (a visitor) also took part in the discussion.

A vote of thanks was accorded to Messrs. Wilmer and Seeley.

Mr. Finlay (President) announced the discovery of a new comet by Davidson, Queensland, on July 22. It was observed at the Cape on July 25, 26, 27, its orbit computed, and cabled to Kiel. The comet has a bright round nucleus, almost star-like, surrounded by faint nebulous matter, and has a short faint tail. It was near the earth when discovered, about one-third of the distance from earth

to sun, and had passed perihelion only $3\frac{1}{2}$ days, and as it is going away now from both earth and sun it will not become a conspicuous object. It is just visible to the naked eye.

Annual General Meeting.

WEDNESDAY, AUGUST 28, 1889.

MR. W. H. FINLAY, M.A., F.R.A.S., PRESIDENT, IN THE CHAIR.

The reports of the Secretary and Treasurer were read and adopted.

The Members present proceeded to the election of a President and Council for the ensuing year, with the following result :—

President : L. PERINGUEY, F.Z.S., Memb. Ent. Soc. Lond.
Council : D. GILL, LL.D., F.R.S., F.R.A.S.
P. MACOWAN, B.A., F.L.S.
H. BOLUS, F.L.S.
W. H. FINLAY, M.A., F.R.A.S.
F. GUTHRIE, LL.B.
R. MARLOTH, M.A., PH.D.
R. TRIMEN, F.R.S., F.L.S., F.Z.S.
HON. C. A. SMITH, M.A.
J. H. M. BECK, M.B.
REV. G. H. R. FISK, C.M.Z.S.

Mr. Finlay then delivered the President's address, at the conclusion of which a vote of thanks was proposed by Sir H. de Villiers and carried unanimously.

REPORT OF THE PROCEEDINGS OF THE SOUTH AFRICAN PHILOSOPHICAL SOCIETY

During the Year Ending 1889, July 31.

1. Since the last Annual General Meeting nine Ordinary Monthly Meetings have been held. The average number of Members present has been ten, and of visitors eight, making an average total attendance of eighteen.

2. At the Ordinary Meetings ten papers have been read in the subjects Anthropology, Astronomy, Engineering, Geography, Geology, Meteorology and Zoology. Notes and shorter communications on a variety of subjects have also been brought before the Society; brief accounts of these will be found in the Notes of Proceedings.

3. Twenty-six presents of books have been received. A list of these will be found in the Proceedings of the Society. There has been a considerable demand for the publications of the Society lately, and a corresponding increase in the number of presents received. A bookcase has been placed in the meeting room, one key to which is kept by the Secretary and the other at the Commercial Exchange. Access can be had to the books by Members at any time on application to Mr. Ellis at the Commercial Exchange. In case of a member taking any book away notice should be given to the Secretary.

4. During the year ten Ordinary Members have been elected and one has resigned. The total number of Ordinary Members is seventy-six, and of Corresponding Members twenty-three.

5. Volume IV. part 2 and Volume V. part 1 have been issued to Members and distributed to a number of Scientific Societies in various parts of the world.

DAVID GILL,
General Secretary.

Dr. The Treasurer in Account with the South African Philosophical Society for the year ended 30th June, 1889. Cr.

1888.	To Balance :	£	s.	d.	£	s.	d.	1888	By assistance to Treasurer	£	s	d.	£	s.	d.
July 1	Mortgage Bond ...	200	0	0				July.	and Secretary ...		...		27	13	4
	Bank Balance ...	89	9	9					„ Printing Account, Post-cards, &c.	2	5	10			
					289	9	9		„ Printing 500 Copies of Transactions ...	52	6	3			
	„ Subscriptions ...	168	0	0					„ Printing *rê* Cape Orchids (500 copies)	51	19	1			
	Overpayments thereon ...	0	4	0									106	11	2
					168	4	0		„ Rent room of Exchange	15	0	0			
	„ Interest on Mortgage Bond ...		...		14	0	0		Book Case for Meeting room	8	0	0			
	„ Sale of Copies of Transactions ...	0	17	6					„ Sundries, viz.: Postage,				23	0	0
	„ Sale of Copies of Transactions *rê* "Cape Orchids"	12	19	0				1889	Stamps, Railway fares, &c.		...		9	6	4
					13	16	6	June 30.	„ Balance : Mortgage Bond	200	0	0	166	10	10
									Bank Balance ...	104	0	5			
									Cash received on 30th June, but not deposited until 1st July	14	19	0			
													318	19	5
					485	10	3						485	10	3

Cape Town, 22nd July, 1889.

C. ABERCROMBIE SMITH, Hon. Treasurer.

We, the undersigned, Members of the South African Philosophical Society, hereby certify that we have examined the above account, compared the cash payments with the vouchers and the balance with the Bank Pass Book and Mortgage Bond, and found the same to be correct.

Cape Town, 22nd July, 1889.

JOHN E. B. ROSE,
W. HAMMOND TOOKE, } Auditors.

ANNUAL ADDRESS TO THE MEMBERS OF THE SOUTH AFRICAN PHILOSOPHICAL SOCIETY,

On the 28th August, 1889,

By the President, W. H. Finlay, M.A., F.R.A.S.

It has usually been the custom in our Society for the President at the end of his second year of office to review the work accomplished during the two years of his presidency. As however my two immediate predecessors in this chair found it irreconcilable with their other duties to accept the honour of re-election at the end of their first year, it falls to my lot to review our work for four years in order to maintain continuity.

We have had a large number of interesting exhibits at our meetings which have elicited much useful information from members. In the first rank of the exhibitors must be placed the Rev. G. H. R. Fisk, whose energy in this direction I would like to see imitated by many more of our members. Among Mr. Fisk's exhibits I must specially refer to a complete series of the *homopus* tortoises, viz., *homopus areolatus*, *hom. signatus*, and *hom. femoralis*, the only complete set which has I believe been shewn alive, and of which *hom. signatus* was new to the collection of the Zoological Society of London. Also to a second specimen of *testudo Fiski*, the only other one known having been discovered by Mr. Fisk towards the end of 1885. The discovery of this specimen and of *testudo Trimeni* led Mr. Boulanger to a complete revision of the South African tortoises allied to *Testudo geometrica*, with the result that seven well-marked species could be distinguished. Mr. Fisk had also the pleasure of discovering a new snake which was described by Mr. Boulanger in the Proceedings of the Zoological Society of London May 3, 1887, and was named *Lamprophis Fiski*.

Mr. Trimen shewed us a perfect specimen of the rare Lycoenide butterfly, Aphnoeus Hutchinsonii. This species was previously founded on a single worn example of the male captured in 1886.

To Mr. Péringuey we have been constantly indebted for exhibits of beetles and for explanatory remarks on them. One recently shewn by him, a small tiger-beetle (Ciandela Nilotica) has a very wide *habitat:* it has been found from Cairo to the Blue Nile, also in Senegambia and the specimen shewn was found by Mr. Carrington-Wilmer for the first time in South Africa.

Mr. Bolus exhibited a specimen of *vaccinium exul* which he had found about thirty miles from Barberton, which was very interesting as being the only plant of the order known in extra-tropical Africa; no *vaccinium* having hitherto been recorded south of the Tropic of Capricorn in any part of the world.

To Mr. MacOwan we are indebted for a specimen of Ixianthis retzioides, a rare and little known plant. He was fortunate enough to procure a number of specimens and a good supply of ripe seed, so that we may hope that this fine plant will be introduced to cultivation. Mr. Woods shewed us some photographs of lightning-flashes which he had taken during a thunderstorm. The appearances presented were curious and perplexing and it is highly desirable that further investigations should be made. Unfortunately for us (*i.e.*, so far as the study of these flashes is concerned) our opportunities are few at the Cape for such work, but perhaps some of our friends in the Eastern Province will take up the question. The interest attaching to such photographs at the present time is very great as it has been recently asserted that in some kind of storms the duration of the flash is not by any means infinitesimal.

Among the papers presented to the Society and printed in our Transactions the most important are Mr. Péringuey's second contribution to the South African Coleopterous Fauna and Mr. Bolus' orchids of the Cape Peninsula. Mr. Péringuey's paper is in continuation of the one published in Vol. IV. of our Transactions: the present contribution gives an exhaustive description of 187 Coleoptera, several of which are new to science and the paper is illustrated by four plates, figuring forty-five specimens. The previous contribution described 100 specimens and I believe that Mr. Péringuey has a third one well advanced. Mr. Bolus describes botanically 102 orchids growing on the Cape Peninsula, arranging them in groups and stating where they are to be found and their period of flowering. It will be seen that several of the species described were new to science when discovered by Mr. Bolus or his botanical friends, among whom I am glad to say are two of our members, Prof. A. Bodkin and Dr.

Marloth. In fact out of the 102 species therein enumerated thirty-one had never been previously recorded as occurring in the Cape Peninsula and of these fifteen were new to science. The book is enriched with thirty-six exquisite drawings, partly coloured, of orchids which, with the exception of four or five, have never been previously figured. The immense value of these drawings makes us wish the whole series might have been illustrated in the same way, but the funds of the Society I am sorry to say will not allow of the expenditure, although Messrs. Péringuey & Bolus have borne a large amount of the cost of these plates themselves. Such papers as these, the result of patient work and a profound knowledge of their subject, will enable our Society to take a high rank among the scientific societies of the world.

During the period I am reviewing the presence of Phylloxera Vastatrix was detected at the Cape and we have heard now and again from Mr. Péringuey of the progress made in checking it. Knowing, as we do, Mr. Péringuey's profound knowledge of Entomology it is no surprise to find that his treatment of the Phylloxera and precautions against its spread have received the most unqualified approval from the expert who was invited to report on them in deference to the clamour of an ignorant public. I wonder when the people of this colony will understand that a man who has devoted the best part of his life to a particular study and has attained a reputation in it knows more about it than the hundred and one amateurs who with only a most superficial acquaintance with the subject are ever ready to give their opinion.

Looking at the position of our Society generally I think we may feel satisfied, but not more than satisfied. Our numbers have increased during the last year, but on the average they remain much the same. Owing to the meetings being held in Cape Town the Society is chiefly composed of members who reside there, in fact only ten per cent. of our members reside at any distance from Cape Town. Now this is not as it should be; are there no lovers of science at Stellenbosch, where we have not a single member, Graham's Town or Port Elizabeth? There is, however, a reason for our not numbering many members outside Cape Town, and that is that the subscription is the same for all ordinary members irrespective of place of residence. Consequently a would-be member living at a distance finds that he cannot attend the meetings but that he can buy the Society's Transactions for a few shillings and so he does not join the Society. I

think this might be met by a reduction in the subscription for members residing more than twenty miles from Cape Town, and I intend to bring a motion to this effect before the Council ; if it is carried I hope it will result in a large increase of our numbers.

But I ask the members we have to do more than they have done in the way of providing food for the Society's meetings. I find that out of our present members 22 per cent. have contributed papers of some length during the last four years, and this is perhaps as high an average as can be expected ; but that, including these papers, only 27 per cent. have contributed anything, paper or exhibit, to our meetings. This is a very low proportion, and I hope that members will help us to remove this reproach by bringing more exhibits and by short notes on any point that seems to them worthy of notice. There appears to be a feeling that for a paper to be accepted by the Philosophical Society it ought to occupy at least half-an-hour in reading, but this is an utter mistake. A note of half-a-dozen lines on some interesting phenomenon would be just as welcome and would probably give rise to a valuable discussion.

I should have been glad to bring before you a review of the progress of science in other parts of the world during this time, but am not unfortunately equal to the task ; so that I must ask your indulgence if I act up to the old motto *ne sutor ultra crepidam* and give you some account of the progress in that branch of science with which I am more particularly acquainted.

ASTRONOMY.

The most striking and important advance during the last year or two in this department is the successful application of photography to astronomical *measurements*. Astronomical *pictures* had been taken many years ago, *e.g.*, of the moon, planets and bright nebulae, but it has only quite recently been shown that a photographic plate exposed for a few minutes will furnish as good a position of a particular star with reference to the surrounding stars as a single observation with a large Transit-circle. Of course there are many precautions to be taken in the exposure of a plate to secure this degree of accuracy and laborious researches to be executed on each plate to arrive at the results, but the certainty that this accuracy can be attained will cause a revolution in meridian observing. The meridian circle will be no longer devoted to the observation of vast numbers of stars as has

hitherto been necessary, if anything like a decent knowledge of the starry heavens was to be hoped for ; this work will now be relegated to photography. But it must not be supposed that the meridian circle is superseded and deposed : on the contrary it must return to its more legitimate work, viz., the determination of accurate star-positions. To reduce the stars on any of these plates it is necessary to know the positions of a fair number of the brighter stars on it, and from these the position of all the others may be determined ; but to determine the position of these few standard stars or zero points, we must fall back on the meridian circle. It is certain therefore that in the future the work of the meridian circle will be confined to determining with the utmost possible accuracy the co-ordinates of certain standard stars which will be used as zero points ; observations will be multiplied under conditions calculated to destroy every conceivable systematic error ; more time and care will be given to examining the flexure and instrumental adjustments, with their variations, of the instruments ; more extended and refined investigation of the constants of astronomy will be made, and every true lover of exact astronomy will be rejoiced that more time and study will be available for these questions now that the meridian circle is freed from its uncongenial task, taken up under dire necessity, of zone-observing. And that I do not make these assertions without cause the following facts will prove : If we take the Right Ascensions or Declinations of some standard star as determined from a large number of observations at several first-class observatories we find differences between them ; small certainly, but still larger than should exist. What is the cause of these differences ? Unknown errors of refraction, flexure, personal equation depending on the magnitude of the star, proper motion, &c., &c. The removal of these discrepancies is of primary importance ; many attempts have been made to combine the results of various observations into one system, but as they mainly consist of adopting the results of one or two observatories as eminently free from systematic error and reducing others to them, this plan does not commend itself to my mind as satisfactory. Our distinguished visitor at the Observatory, Dr. Auwers of Berlin, has pre-eminently shone in this work of deducing a standard system of star-places, but even he finds that recent observations at the Cape shew that there are errors still outstanding in our refraction tables. Let then to photography be given the task of determining the relative positions of all stars visible in a telescope and to the meridian circle its proper work of determining zero-points.

PHOTOGRAPHY AND ASTRONOMY.

We have heard so fully from our Secretary, the Astronomer-Royal, about the application of Photography to Astronomy that I shall not enlarge on the subject to-night, but will only refer to one or two points in connection with it. Probably many of you are aware that for several years past regular observations have been made, chiefly at Greenwich and Rugby, to determine the motion of stars in the line of sight, as it is called, *i.e.*, either directly towards us or from us. This is done by measuring the amount of the displacement of some particular line in the star's spectrum caused by a motion of approach or recession. Naturally the amount of this displacement is very small, and owing to this and the tremors in the image caused by the atmosphere, the observations are very difficult and liable to large accidental errors; indeed, I remember a distinguished astronomer telling me it was the most difficult observation he had ever tried to make. This being the case we have had to be content with results differing widely among themselves but probably something like right in the mean of a large number. For example, here are some Greenwich results for the motion in miles per second of Capella: + 4, + 19, + 52, + 38, + 53, − 4, − 55, + 42, − 21, + 2, + 30, the mean of all being + 16.

Quite recently Dr. Vogel, at Potsdam, has photographed the spectrum of this star together with that of an artificial light, and the results of his measures on ten nights vary only from + 14 to + 18.

The agreement between his results and the mean of the previous ones is satisfactory, but what a difference in the accuracy of the individual measures! What a gigantic stride in advance are these photographic results over all that had preceded them! Now at least we may feel sure of arriving at a definite knowledge of the motions of approach or recession of the brighter stars with a fair prospect of accuracy.

THE SOUTHERN SKIES.

The preliminary photographic survey of the Southern Skies which has for the most part been carried out at the private expense of the Astronomer Royal at the Cape, was completed a few months ago, and the positions of the stars are being measured from the plates by Prof. Kapteyn of Groningen. It is satisfactory to know that the plates bear the test of measurement well, and the result will be a catalogue of all the stars in the southern hemisphere down to $9\frac{1}{2}$ magnitude, the

place of each star being correct to within about 5″. You are all aware that the great international scheme for securing a photographic chart of the whole sky down to the 14th magnitude has been successfully floated and that the Cape will have a share in the work ; our observatory and dome are up at the present time and work will be commenced in a few months. With the new telescopes, exposures of a few minutes will be sufficient to secure stars to the 14th magnitude, and exceptional precautions are taken to secure accuracy.

I will only mention one other result of recent photography, viz., in connection with long exposures. A good many years ago Prof. Tempel announced that he could see a faint large nebula extending to a considerable distance from the star Merope in the Pleiades ; such an announcement naturally caused the most lively interest, but the evidence in favour of its visibility was most conflicting ; some of the best observers with large instruments were utterly unable to detect anything of it, and again it was seen and sketched similarly to Prof. Tempel's description by others. The balance of probability, however, I think, was in favour of its non-existence. A few years ago Mr. Isaac Roberts, a Liverpool merchant who has devoted his spare time to astronomy, took up the subject of astronomical photography with the best instrumental adjuncts that money could supply. He has devoted his attention chiefly to the results of long exposures and has reaped truly astonishing success. With an exposure of from three to four hours he finds that not only is the Merope nebula a reality, but that the whole region of the Pleiades is swarming with nebulous matter which is quite invisible to the naked eye ; the photographic plate recording the accumulated energy of these faint masses of light which are quite unable to affect the retina of the eye.

Mr. Roberts has since shown some wonderful results from long exposure and there is no doubt but that there is a rich harvest to be gathered in this direction.

AT THE OBSERVATORY.

The new Heliometer at the Observatory has been at work now for about a year and a half : it has been employed chiefly by Dr. Gill for the determination of stellar parallaxes. With it he has accumulated material for the determination of the parallax of a dozen stars varying from the 1st to the 4th magnitude, and a similar work has been carried out at Yale College Observatory for the Northern

hemisphere by Dr. Elkin. The Heliometer was also used last October and November for a determination of the sun's distance by observations of the minor planet Iris by Dr. Gill and myself, and again during the last three months of the minor planet Victoria by Drs. Auwers and Gill, who have secured over 3,000 observations. The degree of accuracy that can be secured with this instrument is truly astonishing, and if we are fortunate enough to secure a sufficient number of corresponding observations in the Northern hemisphere I am confident that the vexed question of the Sun's distance will be settled within very small limits from these observations.

As an instance of what this instrument is capable of, I will quote some results given by Dr. Gill in a paper read before the Royal Astronomical Society in 1888. He gives some measures in that paper of β Centauri from a couple of faint stars at nearly equal distances on opposite sides of it, and all he is concerned with for the determination of the parallax is the difference of these distances ; but taking the sum of these distances amounting to about 10,000 seconds of arc the probable error of this very considerable distance from a single observation is only $0.''2$ of a second of arc.

GEODETIC SURVEY.

The Geodetic Survey of South Africa has been gradually progressing under the able management of Major Morris, R E. I mentioned in my address last year that an unexpected discrepancy had been found between the Geodetic and Astronomical latitudes in the neighbourhood of Port Elizabeth. This has been most carefully investigated by Major Morris since then and he finds that the discrepancy extends to several places in the neighbourhood. Port Elizabeth and Coega Kop shew discrepancies of nearly the same amount and sign, while Zuurberg and Berlin shew as much discordance in the opposite direction. It would seem that somewhere within the area included by these points the density of the underlying strata is considerably greater than the average density of the earth : the station at Driver's Hill seems to be very near the line of no discordance. Similar anomalies have been found in many parts of the world, but still it is to be hoped that this one will be investigated more fully. A determination of the longitudes of the most important ports on the West Coast of Africa is being carried out at the present time by Commander Pullen, R.N., and myself. Observations for time are

made at each station and then clocks are compared on the same night by signals through the Western cable, which has been placed at our disposal for this purpose by the kind courtesy of Sir Jas. Anderson. The observations for the longitude of Port Nolloth are already finished, and I am at present engaged with those for Mossamedes. In this way the longitudes of Mossamedes, Benguela, St. Paul de Loanda, St. Thomé and Accra will be determined accurately: and by signals from St. Vincent it is hoped that a check will be obtained on the difference of longitude between the Cape and Greenwich.

COMETS.

During the period I am considering, two comets have been discovered at the Cape by two of our members, one by myself in 1886, September, and one by Mr. Sawerthal in 1888, January. The one that I had the pleasure of finding was always a faint telescopic comet; it proved to be a periodic one revolving in an orbit of $6\frac{2}{3}$ years' period, and moving in a path very similar to the comet discovered by De Vico in 1844. The two comets, however, do not seem to be the same. Mr. Sawerthal's comet was just visible to the naked eye when found, and presented some very interesting changes of form during the time it was visible; its orbit seems to be elliptic but the periodic time is over 2,000 years. Now the discovery of these two comets by men who have little time to give to such work as searching for them gives me the opportunity of making some remarks that have often been in my mind. How is it that in a climate so peculiarly suited for astronomical work as that of South Africa we have practically no amateur observers? Mr. Eddie, of Graham's Town, has a good telescope and is always to the fore when a bright comet turns up; but with this exception I do not know of any amateur observer in South Africa. The climatic conditions are all that could be desired, the nights are never unbearably cold—indeed it is often a relief to be out of doors instead of inside—and yet there is no one to be found with a sufficient liking for astronomy to make a series of observations. I do not wish you to understand that I am advocating the immediate formation of a Cape Astronomical Society, though perhaps it will come in time; for the present I think the pages of our own Society's publications will suffice to place on record any observations that may be made; but I certainly do wish that some means could be found to arouse a more active interest in the oldest of the sciences.

METEOROLOGY.

Our knowledge of the Meteorology of the Colony is steadily increasing : there are now forty-one stations equipped with a full set of instruments and 317 rain-gauge stations. The set of thermometers by which a record of the temperature at each hour of the day is obtained is at work in King William's Town, after having been observed for two years at Van Wyk's Vlei. As the result of its stay at the latter place we now know how the average temperature varies during the twenty-four hours in the various months of the year. Previously the only place where this was known was at the Observatory ; and as was expected, the temperature curve for the Karoo differs considerably from that for the neighbourhood of Cape Town ; for example here the hottest part of the day is about 1.30 p.m., while at Van Wyk's Vlei it is between 3 to 4 p.m.

The papers read by Mr. Howard and Prof. Guthrie on the reduction of barometer readings to sea-level are of great value and must receive special mention. This reduction, most difficult in a country like South Africa but yet essential for any enquiry into isobaric curves, has for many years occupied the attention of the distinguished meteorologist, Prof. Loomis, in America ; and it seems as though in every country the peculiar factor suited to that country must be sought ; I believe that Messrs. Howard and Guthrie are continuing their investigations in this direction.

A valuable contribution to our knowledge has been recently published by Dr. Karl Dove, viz., the climate of extra-tropical South Africa, which is founded to a large extent on the results accumulated and reduced by the Meteorological Commission. He divides the country into a number of belts over which the meteorological conditions resemble one another, and for each of these he discusses the variations of temperature, rainfall and humidity of the air throughout the year.

There is one point in connection with the published results for the humidity to which I should like to draw attention, that is that they all make the amount of dampness too great. For the convenience of the observers, who are all volunteers, the hour of observation is fixed at 8 a.m. local time. Now at this time, even close to the sea, the degree of saturation is greater than for the mean of the day by 4 or 5 per cent., and I am sure that this amount is considerably exceeded at inland stations. I was particularly struck with this during some short

visits I paid to Matjesfontein last winter, where the mean degree of saturation derived from the 8 a.m. observation for the months of July and August was 84 per cent. but in an hour or so it would generally fall to about 60 or 65 and remain so till night. In fact one's feelings utterly negatived any such degree of saturation of the air as 80 per cent. I do not believe that this increase of dryness is cancelled by a corresponding decrease during the night, and even if it is the main requirement for a health-station is that the air should be dry during the day-time. I consider, then, to arrive at anything like a proper estimate of the value of health-resorts up-country an observation of the humidity should be made at at least one other hour in the day. In conclusion, I will quote some remarks of Dr. Dove in connection with the average amount of clouded sky in the South-Western district. He says "We cannot refrain from a comparison of the amount of cloud here with that of the far-famed sky of Italy. According to Hann the mean amount of cloud in Upper Italy amounts in winter from 5 to 6, and sinks in summer from 3 to 4, in Southern Italy from 2 to 3. Even the neighbourhood of Cape Town then is distinguished by a still smaller amount of cloud. Yet the Europeans of South Africa possess neither an art nor a literature of their own: the reason lies in their undeveloped state and home relations (in den unentwickelten politiken und wirtschaftlichen Verhältnisse). When future generations have fostered these branches of human knowledge in that part of the world also, then we shall just as little miss in their productions the influence of the bright sky, strengthened by the unbroken mildness of the climate, as among the equally-favoured nations of South Europe."

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THE TRANSACTIONS

OF THE

SOUTH AFRICAN PHILOSOPHICAL SOCIETY.

THE WINTER STORMS OF SOUTH AFRICA, ILLUSTRATING THE VALUE OF CAPE POINT AS A WARNING STATION.

BY

A. G. HOWARD.

[Read 28th September, 1886.]

On entering into the question of the meteorology of Cape Point, it is intended to show how an isolated peak of medium altitude, intercepting the path of a cyclone, may be utilized as a warning station to places further to the west and in its immediate vicinity.

Before discussing this question, it will be necessary to glance at the atmospheric currents of the earth generally. Roughly speaking, the earth can be divided into a series of zones of which the following is the general summary :

An equatorial zone of calms. Two trade wind zones. Two semi-tropical zones of calms, and two return wind zones. This is the general geography of the winds of our globe, the only portion which will affect us to-night being the trade and semi-tropical zones of the southern hemisphere, more particularly applying to South Africa.

About the latitude of Cape Town is situated that high pressure belt from which the south-east trade winds blow : this south-east trade current carries the atmosphere to the equatorial region of calms, where it rises and flows back again as an upper north-west current, bearing the name of the upper return trade wind.

The altitude of this current must be very great indeed near the region of calms, for Smythe when at Teneriffe at a height of 12,000 feet above sea level, was still in the region of calms dividing the two

currents, and saw cirrus cloud at a great height above him floating from the equator.

But we in South Africa are very differently situated to what Smythe was at Teneriffe.

The belt of high pressure, which I mentioned before as being about this latitude, is where the upper current reaches the level of the earth, and as this belt is south of us during the summer, and abreast of us during winter, the return current cannot be very high up. From observations, I have come to the conclusion that the summer level of the lower portion of this current is about 3,000 feet above the sea, or slightly higher than the top of the Lion's Head. Table Mountain and most of the peaks of that range penetrate into this current, which is the cause of the table cloth being spread over these mountains when the south-east wind blows, of which I will speak more anon.

Now the altitude above sea level at which this return current will be reached depends greatly upon the nature of the country. A gently sloping land surface, gradually increasing in altitude as the centre of the country is reached, repeats itself at the lower surface of this current. Thus the height from the land surface at Kimberley to this upper current will be approximately the same as at Port Nolloth which is at the sea level. This can be proved by noticing that the same cyclonic movements which affect the lower stratum of our atmosphere are felt at all the stations over Cape Colony, although some of those stations are over 4,000 feet above the sea level, whereas did the return current flow at an uniform level, it would flow round the Colony like water round a rock, and the atmospheric conditions of high stations would be totally different to those at sea level ; and this, most certainly, is never the case.

But where there is an isolated peak the conditions are different, as in this case there is no gently graduated contour of ground, but an abrupt elevation round which the atmosphere flows ; thus the atmospheric phenomena may be entirely different at the apex of such a peak to what they are at the base.

Cape Point is such a peak, standing up about 816 feet above sea level, and offering itself as an indicator of coming storms to those who will take the trouble to investigate what its warnings portend.

Before going any further we must come to some decided understanding as to the physical conditions of a cyclone. What is a cyclone ? A cyclone is a portion of our atmosphere where the winds circle round a definite centre ; at this centre the barometer reads lowest, and at

varying distances outwards, equal barometric readings form a series of rings round this : the outer ring having the highest reading, and the rest being graduated towards the centre.

The wind does not circle round a cyclone in true circles at all altitudes.

Near the sea level the wind forms the greatest in-going angle with the isobars and this angle gradually decreases, till at an altitude which varies with its position with reference to the cyclone the wind flows parallel to the isobars. Above this point the wind has a gradual out-flowing tendency, which reaches its maximum at the point where the cyclonic influence ceases.

Below this turning point of the wind's direction, clouds are very seldom seen, but when anything approaching to a cloud is formed, it takes the appearance of fog. Hence I call this, the fog bearing stratum. The next region of the atmosphere where the winds have a tendency to circle outwards is the stratum of the very lowest clouds. Above this is the great upper current. This current can be divided into two distinct portions ; the lower, which is considerably modified by the direction of the cyclonic centres, and the upper portion which from day to day, month to month, and year to year always flows from some point near north-west, and which is in fact the true compensating upper return current of the trade winds.

The former of these is the stratum where the heavy cumulus and cumulo-stratus cloud is formed, while the latter is the home of the cirrus cloud.

Now if the whole solid contents of a cyclone could be rendered visible to one situated externally to it, what would be its general appearance ? I will try to make this clear. Suppose we take an inverted cone, perpendicular to the surface of the sea. If the apex be immersed in the sea till the circle thus formed round the cone be equal to the surface area affected by the cyclone, and we then cut off an ellipse, the one edge of which shall touch the sea and the opposite edge be elevated considerably above it, the solid figure contained between this ellipse and the sea level will be the true form of a cyclone, providing you dish the surface of the ellipse towards the centre.

The next question which it is but natural to ask is—how can you prove this ? A question which I will try to answer.

I shall answer this question by asking another. Why do cyclones travel from west to east ?

We often hear it said that cyclones travel round the sides of areas of

high pressure. But supposing no such high pressure to exist, what then? And in any case it must be confessed that the barometer is higher in front of a cyclone than it is nearer the centre, and consequently the low must move to the high. If we assume that a low cannot flow towards a high, then a cyclone must stand still unless the high moves forward. But we very often see a high only giving way when a cyclone forces it to do so, the result being some very stiff blowing, shewing that the high dies very hard. Moreover in a great number of cases the cyclone splits the high into two and forms what is now commonly called a "col" or neck. Hence the greatest energy must be in the cyclone, and the initial motive power moving it from west to east must be in itself.

And what is this power? Nothing more nor less than the circulation of winds round the centre.

Let us imagine a cyclone to be situated over the South Atlantic ocean to the west of us. The winds are blowing round the centre in the same direction that the hands of a clock travel round the dial. But these winds are of two kinds, one totally different from the other; for whereas the front wind is an equatorial one, that at the rear is a polar one.

The equatorial wind, or that which blows from the north is a warm one, which causes the atmosphere to increase considerably in bulk for an equal amount of pressure. Moreover this current is heavily charged with moisture. But with the polar wind the converse is the case; for here though the pressure remains the same, the bulk will be much less.

We will now imagine two points at equal distances from the centre, one to the east and the other to the west. At both points the barometer reads 30 inches, but there is a marked difference in the atmospheric conditions of the two places, for whereas at the easterly point the warm and rarified air extends to a great height, at the westerly point the air being denser does not extend to such an altitude. Thus the upper surface (so to speak) of the cyclone forms an angle with the sea level, the line of which if extended to the west would cut the sea at the spot where the cyclonic influence ceases, but if extended to the east would overlap the general surrounding atmosphere and so complete the portion of the cone. It will thus be seen how my figure of the inverted cone is borne out.

From the foregoing it will have been perceived that for equal pressure the air is much more rare on the advance side of the centre than on the following.

In fact the *rarest* portion is that in advance of the centre. The

atmospheric *pressure* is least at the centre, but the air is not the most rarified there.

One of the laws of atmospheric equilibrium is, that dense air always flows towards that which is most rarified and as the air in the advance half of the cyclone is more rarified than that at the centre, it is easy to see how this centre moves from west to east, and as the whole system of winds travels with the centre, the front will always draw the centre onwards.

When a hot stream of water is encountered the cyclone travels along it, because the equatorial air current is more heated there than elsewhere. The Mozambique current is one of this sort, our winter storms generally travelling along it.

But there is another peculiarity as being the result of the opposite winds which affect stations of medium height, and isolated like Cape Point, which will best be described by an example.

Let us once more return to our cyclone which has been patiently waiting over the South Atlantic Ocean. No, not patiently waiting, for while we have been considering its physical conditions the advance winds have reached Cape Point.

Simultaneous readings are taken at the various stations, reduced to 32° and to sea level, and lo ! Cape Point reads too high by about a half a tenth of an inch.

Why is this ? The reading was correct and so was the reduction. Then why this discrepancy ?

If you consider for a moment you will see that such must be the case. Assume the atmosphere greatly rarified, with a sea pressure of 30 inches, then the height of the air column must have been considerably increased, and a greater amount of air must exist above Cape Point than would be the case if the air were more dense, but as the reduction additions to both are the same, providing of course that the temperatures are the same, this will make the reduced reading for rarified air higher than it would be if the air were dense. The converse of all this will be the case on the other side of the cyclone.

Let us now retrogress a little and imagine our cyclone just outside the colony and advancing on to it. Owing to the wedge-shaped form of the advance disturbed portion, isolated peaks will be the first to feel it. Cape Point being such a station we will imagine ourselves there and follow the sequence of the weather. A low pressure lies over the centre of the Colony, the wind at Cape Point is consequently south-east. Below, at sea level, it is south.

Suddenly a haze is seen to the north-west from which long thread-like clouds stream out which gradually spread over the whole sky. The general direction of these thread-like clouds is from north-west to south-east. These are what the Rev. Clement Ley calls *cirro-filum*, of which I will speak again.

Following these threads comes a bank of numerous small rounded cirrus clouds, close together, like an immense flock of sheep. These come driving up from the north-west, and as time passes on they grow larger and larger and seem to run one into the other. Towards the northern horizon they form themselves into a compact white sheet.

At the back of this mackerel sky, a thin hazy film begins to shew. Meanwhile the wind calms down at the peak, while it still blows at the sea. Slowly the vane goes round and a north wind springs up. The advance of the wedge is on us. Let us now take our instrumental readings. They are taken, telegraphed to headquarters, and reduced. The barometer is a half or a quarter of a tenth higher than it should be, while the temperature has risen considerably from what it was at the same time the day before ; the wind too is north, while the general surface current is south and south-east.

Slowly the north wind travels down the sides of the peak till this is the only current observable. The advance of the cyclone is now fairly over the Cape Peninsula, and the usual sequence of falling barometer, changes of wind, &c., follows. When the depression has passed, the wind goes to the west or south-west, often blowing a gale.

Suddenly round goes the vane to south-east again, very often blowing strong while the south-west wind gradually calms down at the sea level. The storm has passed, as the dense atmosphere, too heavy to reach Cape Point, testifies ; that peak experiencing the general trade wind circulation of the atmosphere. But should the vane back to north-west or north, no matter what its direction below, another disturbance will follow. Hence we see how important a station Cape Point might become in the prediction or fore-casting of weather, more particularly the winter storms.

I cannot pass over this portion of my paper without reference to my sun-spot theory. It will no doubt be fresh in the minds of most here that a paper upon this subject was read before this Society in July last, in which I shewed that a relationship existed between the positions of the spots on the solar disk and the various storms which

visit the Cape. Now if further investigations prove this of any practical value, so that we shall be able to tell for a definite period of time beforehand what nature of a storm to expect at even an approximate date, what an important warning station might not Cape Point be converted into. The sun spots will tell to a day or two when the storm is to be expected, and the observer at Cape Point could telegraph at least, and often more than, 24 hours, before its arrival, that it was close at hand.

Having given a general idea of the subject, I now wish to place before you some dry facts and figures. I will confine myself to this year alone. From the first of January to the end of August there were 38 separate cyclones from the north-west, west, or south, purely of the winter type. Taking the state of the barometer on the day of the storm, and on 1, 2 and 3 days before, the following results come out :—

The barometer was high, 11 times 3 days before the storm.

„ „ 7 „ 2 „ „ „ „

„ „ 13 „ 1 „ „ „ „

and 17 times on the morning of the storm.

Thus we find that during this period, twenty-three storms were foretold by an increase of pressure from one to three days before the storm, while seven had an increase *only* on the morning of the storm. The total of storms so foretold is thirty. This leaves eight storms un-indicated by increased pressure within three days of their appearance.

But increased pressure is not the only thing to be guided by ; increase of temperature is another indication, which I think the following will pretty well demonstrate.

Taking the same time as before we find that the maximum thermometer reading at 8 a.m. was 3 days before the storm 7 times.

2 „ „ „ „ 3 „

1 „ „ „ „ 16 „

and on the day of the storm 12 „

Two of the maxima for three days before must be omitted as they were lower than the readings of the days before, but on these days very strong barometer indications are noticeable.

With reference to the wind changes, one reading a day, and that at the very worst time, is hardly enough to construct anything like tables with.

On several occasions the wind blew from a similar direction on the

morning after the change at Cape Point to that at the other stations. This has been put down as a simultaneous change, while in reality the wind might have changed at Cape Point at a totally different time to the other stations. Twenty-four hours is a long period to pass over without an observation.

Roughly speaking the following are the wind changes :—

On fifteen days the wind changed at Cape Point before it did at the other stations.

On fifteen days the change was simultaneous.

And on three days the change did not take place at Cape Point till *after* the lower current had changed. These last three I am inclined to consider as due to wrong observations or to temporary whirls. The remaining five instances are very doubtful as the wind was blowing from the north-west all over the Cape Peninsula most of these times ; hence no change seemed to take place.

From the foregoing, the following inference may be drawn.

When there is either an undue increase of pressure or heat at Cape Point, a westerly depression may be looked for, especially if the wind is blowing from the north there, no matter what the direction may be below.

I have already mentioned that one of the forerunners of a storm is the appearance of a number of thread-like cirrus forms, called by the Rev. Clement Ley, Cirro-filum, but noted down by me long before I ever heard of Ley's name as thread cirrus.

This cirro-filum is formed at the junction of the two strata of the upper current. The upper stratum of this current always flows from some point between west and north-west, but the lower stratum is more or less affected by the cyclonic disturbances, very often travelling from south-west, but more often travelling from a similar direction to its upper component, although I have noticed it to travel from the south, when any great disturbance was at hand or just passed.

Now if a cyclone is advancing from the west or south-west, the advance edge of the cone will affect the motion of this current, the general direction of which will be more westerly. It will also become more charged with moisture. This moisture will form into cumulus clouds with perfectly level bases and sharply defined rounded tops. The more moisture is added to these clouds the higher they will become till, impinging on the upper current, their upper portions will be drawn out into long threads by this upper current travelling more quickly.

A great quantity of the electricity contained in these cumulus clouds will be dissipated into the upper stratum and the particles of water will coalesce and fall as rain. As long as the cloud receives fresh supplies, the cirro-filum will stream off and the rain fall, but as soon as the supply ceases the rain will also cease and the cirro-filum break up into ordinary cirri. As soon as a fresh supply of moisture is received, more cirro-filum clouds will be formed, in their turn to break up. It is thus we see such varying banks of cirri roll over us without a single cloud of medium altitude being seen, those generating clouds being far below the horizon, but surely advancing towards us. An expert if he but knows the point of the compass from which the threads seem to radiate and the direction they are travelling from, can tell exactly what the directions of both currents are.

Cirro-filum is a sure forerunner of rain. For the last two years I have seldom known this indication to fail. And where rain did not follow at Cape Town it was experienced further up-country.

I intend to introduce as a concluding portion to my paper some descriptions of one or two good typical winter storms which have visited us this year, with an account of the indications of their approach.

The first of these is a double depression, one which came from the north-west, immediately followed by a very severe one from the south-west. The whole disturbance extending from the 11th to the 17th August last.

On the 11th the only indication of a coming depression was the thermometer at Cape Point which at 8 a.m. was 3° higher than on the morning before. The general wind was south. On the 12th although the heat was the same, yet the barometer was ·09 of an inch too high, shewing a rapid advance. As the general wind was south, and that at Cape Point south-east, the indications were for a depression from the north or north-west. Next day, the 13th, the heat was 2° higher, while the barometer was ·07 of an inch too low at Cape Point, the wind blowing a south-east gale. By the low barometer the first depression had passed, and by the high thermometer another one was not far off. On the 14th neither barometer nor thermometer gave any indications, but the prevailing wind was north-west, while at Cape Point it was north-east ; when the exact changes took place of course I cannot say, but the indications were for an approaching cyclone from the south-west. On the 15th the

thermometer at Cape Point had risen 2° and the barometer read ·09 of an inch too high. The general wind was north, being fresh at Cape Point. On the 16th the storm was on us in all its fury. On the 17th this depression was passing away to the east and an anti-cyclone forming over the Colony.

The first of these two was a dry depression, as far as Cape Town was concerned, no rain falling. The first rain that fell was on the night of the 14th in advance of the second depression.

This second storm shewed itself to Cape Town observers by means of the upper currents while the first one was advancing on us, for on the 11th cirro-filum was developed all over the sky radiating from north-west to south-east, remaining there all that day and a part of the 12th, after which the sky remained clear till the 14th, when it became overcast. On the 15th at 8 a.m. my gauge registered ·055 of an inch of rain, but on each of the following mornings three-tenths of an inch was measured, while on the 18th as the storm had passed, only ·16 of an inch was gauged. The self-recording anemometer at the Royal Observatory bears out the foregoing description of the tracks of these two storms, as also do my own daily charts.

The next example I shall give will be that of a storm which I predicted two days before, merely by noticing the direction of the clouds, in the lower stratum of the upper current.

On the morning of the 21st of August last, a well-defined depression of the summer type lay over the Colony. The wind over Cape Town was south. Heavy low damp stratus clouds were driving from the south, while above Table Mountain some heavy cumulo-strati were moving at a moderate speed from the west. What made these clouds move from the west when the general direction was south? Nothing could do so but the advance wedge of an approaching cyclonic cone, and that cyclone must have been to the south. Hence I noted down the prediction that a depression was to the south of us and would strike the Colony in about two days. On the 22nd the barometer had fallen considerably, but this was due to the near approach of the northern cyclonic centre. The thermometer at Cape Point had risen 7° from the morning before, the wind there being east: at that time, (that is 8 a.m.) it was veering to the north. During the afternoon the barometer at Cape Town reached its mimimum, and a nor-westerly wind sprang up. As the barometer began to rise slight showers fell which continued during the night, the wind freshening. On the morning of the 23rd the centre of the depression was off East

London, the whole Colony to Kimberley and Bloemfontein being affected by it. Fresh north-west and west winds blew all over the Colony.

The rainfall in Cape Town was moderately heavy, being ·36 of an inch in my gauge on the morning of the 24th. On the whole of the 23rd, in fact, the weather was very stormy and squally, especially during the evening when some very heavy squalls were experienced. On the morning of the 22nd the Cape Point barometer read ·02 of an inch higher than it should, which indicated another depression and sure enough during the day a small secondary passed.

One peculiarity of the wind at the Royal Observatory as self-recorded is that from 7 a.m. till midnight on the 23rd the wind although generally calm as registered by the mileage trace, yet passed in a series of squalls which kept on continually altering the direction of the vane from north-west to west, back to north-west then to south-west and south-east, back through west to north-west and repeating itself over and over again many times each hour.

It is in cases like these that the anemometer trace fails to indicate the intensity of individual gusts. One gust at half-past-eleven is visible on the trace as a very slight horizontal line. The speed at which the cups must have revolved for this short time must have been inconceivable.

There are a few more of these faint horizontal marks, but the one I have mentioned is the most conspicuous.

I could go on multiplying cases if I liked, where storm after storm passed, but there would be such a sameness about each one that it would become very monotonous, as the general sequences are the same, the only variation being due to the directions they come from, and the tracks of the centres. If they come from the north-west they are preceded by south-east winds which suddenly go round to the north-west as the centres pass; but if they come from the south-west, they are preceded by north-west winds, which chop round to the west as the centres pass.

These latter are the ones which bring the most rain to this end of the Colony. The reason of this is that the left-hand half of the cyclone is the wet half, so that if a storm comes from the north-west and passes us to the north-east, we are the whole time in the dry half of the cyclone.

At the beginning of this paper I mentioned that the Table Cloth on Table Mountain was due to the presence of the lower stratum of the

return equatorial current at a level of 3,000 feet above the sea. I promised to speak more on this subject and I may as well introduce it here before concluding. You will remember I said that when the contour of a country was gradual the atmospheric currents did not flow round but up it. It is thus with the backbone of the Cape Peninsula. From Cape Point the slope is gradual to the high table land behind Simon's Town. From there it is comparatively level (with the exception of the slight depression at Noord-Hoek) right on to the Constantia and Muizenberg range. From Hout Bay the slope is gradual up to the back of Table Mountain and the Twelve Apostles range.

Thus when a south wind blows round this end of the Cape Colony, the wind instead of flowing round the Cape Peninsula is carried up the sloping backbone before mentioned and impinges into the lower portion of the return current.

Now as this return current is warm and charged with moisture, the cold dry southerly current rushing into it condenses the moisture into the heavy cumulo-stratus cloud which we commonly called the Table Cloth.

As the wind whirls over the cliffs of Table Mountain it once more reaches the lower and colder stratum ; the particles of cloud meanwhile having a propulsive and upward tendency are soon dissipated, the general appearance being that of a cloud constantly being formed and as constantly disappearing, a phenomenon familiar to us all. This is briefly the cause of the south-east cloud.

When the high pressure belt is further to the south and the return current exists *above* Table Mountain, or else when the general currents are southerly, no cloud can form. We have then what is commonly called a *blind south-easter.*

A black south-easter is not a south-easter at all but a south-wester, and is generally the following wind of a winter depression.

There is still one investigation which would be very beneficial to the study of the tracks of winter storms, and that is, the annual position of the South Atlantic anticyclone. The position of this has a great influence on the direction from which the storms come to the Colony, and as this direction affects the rainfall, it will be seen how important it would be to be able to tell exactly for a year or two in advance, from which direction the storms would come upon us.

I have no doubt that when this is investigated it will be found to have a cycle agreeing with the sun spot period of about eleven years.

I do hope that someome will be stirred up with an ambition to solve this problem, which is of such importance to a colony depending for its prosperity on its rainfall. Paper after paper has been read on the subject of South African Meteorology, and still no one seems stirred with any desire to further its study, except a few who have to struggle alone and overcome difficulties greater than most people imagine exist, for I do not believe there is a more disheartening study in the world than that of Meteorology, and especially that of South Africa.

ADOLPH G. HOWARD.

September, 1886.

CAPE POINT.

Height of Thermometer, one, two and three days before a winter Storm, and on the morning of the Storm.

No. of Storm.	Days before Storm.			Day of Storm.
	3	2	1	
1	63	63	66	65
2	66	68	65	69
3	65	65	67	65
4	64	65	64	70
5	63	62	64	66
6	60	64	67	67
7	58	59	60	58
8	60	55	58	60
9	60	62	67	61
10	62	64	68	58
11	57	58	59	59
12	57	60	64	No record
13	63	56	60	68
14	57	56	56	68
15	68	56	61	59
16	53	52	53	53
17	53	54	50	54
18	50	54	58	60
19	60	54	51	50
20	49	52	53	53
21	53	52	55	55
22	57	51	53	55
23	55	54	62	54
24	55	51	60	64
25	62	56	57	48
26	49	50	58	51
27	51	54	52	53
28	52	53	50	52
29	48	52	54	58
30	50	50	59	53
31	53	53	52	50
32	50	45	45	53
33	58	56	62	70
34	52	52	51	51
35	46	49	49	51
36	51	50	52	52
37	51	43	50	50
38	50	49	53	53
	56·1	55·2	57·5	56·2

CAPE POINT.

Days on which the wind changed (x) at Cape Point and (o) generally over the Cape Peninsula, one, two and three days before the storm, and on the morning of the storm. As only one reading is taken a day (8 a.m.) it is hard to tell when the wind really changes, so that this can only be regarded as an approximation.

No. of Storm.	Days before Storm.			Day of Storm.	
	3	2	1		
1	...	...	...	x o	
2	...	...	o	x	
3	.	x	...	o	
4	...	...	x	o	
5	...	x o	...	...	
6	...	...	x o	...	
7	...	. .	o	x	
8	.	...	...	x o	
9	x o	..	...	...	
10	...	..	...	o	
11	...	x o	...	...	
12	x	...	o	...	
13	...	...	..	x o	
14	...	x	...	o	
15	...	...	x o	...	
16	...	...	...	x o	
17	...	...	x	o	
18	...	...	x	o	
19	...	Undecided		...	
20	...	x o	. .	...	
21	...	...	x o	...	
22	...	o	x	...	
23	...	x	...	o	
24	...	...	x	o	
25	N.W.	all the	time	...	
26	...	x	...	o	
27	o	C.P. C	hanged 4	days bef	ore
28	N.W.	all the	time	...	
29	...	x	No Ch	ange bel	ow
30	x	...	...	o	
31	...	...	x o	...	
32	...	...	...	x	No change below
33	x	...	...	o	
34	...	...	x o	...	
35	...	No Change		...	
36	...	x o	...	...	
37	...	...	x	o	
38	...	...	x o	...	
	1 x o	4 x o	6 x o	4 x o	
	3 x	5 x	6 x	3 x	
	1 o	1 o	3 o	12 o	

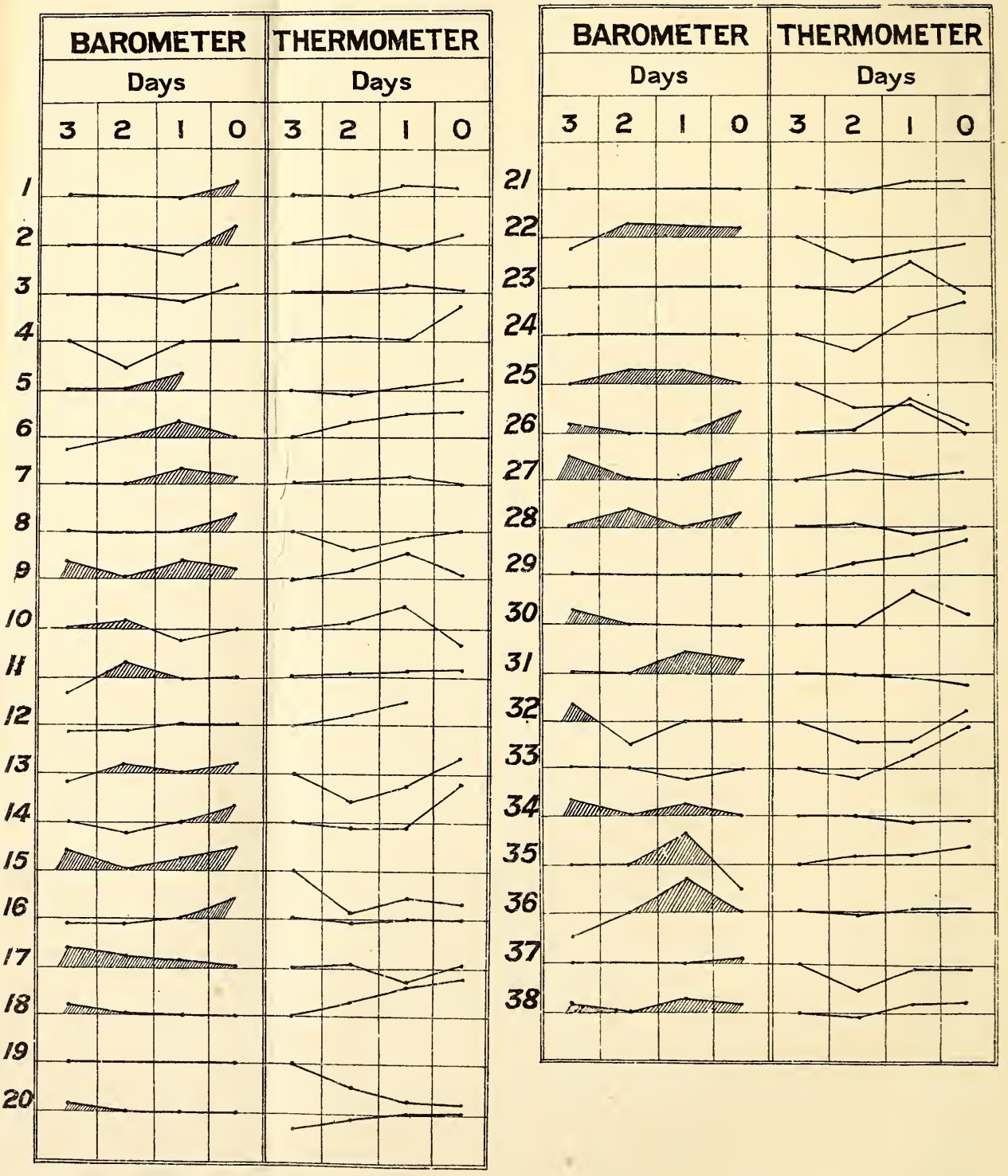
CAPE POINT.

Differences between barometer readings reduced to sea level and as indicated by the isobars, on one, two and three days before a winter storm, and on the morning of a storm.

No. of Storm.	Days before Storm.			Morning of Storm.	
	3	2	1		
1	...	...	...	+·04	
2	...	...	−·02	+·05	
3	...	...	−·02	+·02	
4	...	−·07	...	...	
5	...	...	+·05	No record	
6	−·03	...	+·04	...	
7	...	...	+·04	+·02	
8	...	...	...	+·05	
9	+·05	...	+·05	+·02	
10	...	+·02	−·03	...	
11	−·04	+·05	...	...	
12	−·02	−·02	...	...	
13	−·02	+·02	...	+·03	
14	...	−·03	...	+·05	
15	+·05	...	+·03	+·07	
16	−·02	−·02	..	+·05	
17	+·05	+·03	+·02	...	
18	+·02	..	...	...	
19	...	...	...	...	
20	+·02	...	...	...	
21	...	...	..	...	
22	−·03	+·03	+·02	+·02	
23	...	..	...	...	
24	...	...	...	...	
25	.	+·04	+ 04	...	
26	+·03	...	...	+·06	
27	+·06	...	...	+·05	
28	...	+·05	...	+·03	
29	...	...	...	...	
30	+·04	...	...	...	
31	...	...	+·06	+·04	
32	+·04	−·06	...	...	
33	...	...	−·02	...	
34	+·04	...	+·03	...	
35	...	..	+·09	−·07	
36	−·07	...	+·09	...	
37	...	...	...	+·02	
38	+·02	...	+·04	+·02	
Totals	11	7	13	17	Too high
Totals	7	5	4	1	Too low

CAPE POINT.

Diagrams shewing (1) differences of Barometer Readings, as reduced to sea level, from what they are indicated by the Isobars; and (2) the Readings of the Thermometer, on 1, 2, 3 days before and on the day of the Storm.



Scales. Barometer $\frac{1}{5}\frac{in}{11} = \frac{1}{10}\frac{in}{11}$
Thermometer $\frac{1}{5}\frac{in}{11} = 10°$

ON VAN WYK'S VLEY RESERVOIR.

By J. E. Macnellan, C.E.,

[Late Resident Engineer on Van Wyk's Vley Works.]

[Read 27th October, 1886].

Before proceeding with the main subject of this paper, it may not be out of place to make a few remarks on Boer dams in general, showing that more care should be taken in the selection of their sites, and in their mode of construction.

Many of these sites are far from being good ones, some having such small catchment areas that they never half fill, and their constructors have the mortification of seeing a huge bank, with little water, besides being put to much unnecessary expenditure; others again are constructed on a porous stratum through which the water percolates away rapidly, rendering the dam almost useless for irrigation purposes and only giving water for stock for a comparatively short time. These faults can be avoided by making a proper survey with levels, and by sinking pits along the proposed line of bank, to ascertain the nature of the strata forming the foundation.

A common fault in many of these dams, is that the inside or water slope is made too steep, causing the pitching to get loose and fall out in many places, thus giving free access to the wash of the water, and to the ravages of crabs. This if not noticed and repaired at once, may often cause the entire destruction of the bank, and the loss of a whole season's water and crops.

More care should be taken to incorporate the bank with the ground on which it is to be made. This is done by removing all loose stones, sand and bushes, and picking the ground to a depth of eight inches or so, before beginning the first layer of the bank.

The general method adopted amongst the farmers in the construction of their dams is to convey the ground to the site of the bank, in carts or scrapers, allowing it to assume its natural slope, or angle of repose. This for most kinds of ground is quite insufficient, when exposed to the action of rain or standing water. A farmer once

called on me and said he had just entered into a contract for the construction of a reservoir bank on his property for a certain sum, and now wished to know how much earthwork would be required to finish the work, insisting, in spite of my remonstrances, on only giving such slopes as the material would stand at when first tipped. The slopes were not to be pitched, owing to the scarcity of stones. It will scarcely be a matter of surprise, if this dam, so constructed, should be washed away by the first heavy rain.

Another fault in many of these dams is that too little attention is paid to their overflows, and this is a very serious one, for the existence of the bank depends almost always on a proper provision being made to carry off the surplus water after the dam is full. If this point is neglected the water will rise till it reaches the crest of the dam, when it then only becomes a question of time for it to share the fate of many others whose safety valves have been omitted or made too small.

It is scarcely a matter of wonder to hear of so many dams having given way after a heavy rainfall, when they are made in this haphazard style by men who have little knowledge how to construct a bank able to withstand the denudation caused by alternate filling and emptying as well as by the insidious percolation of standing water.

Outlets should, where practicable, be placed some distance from the bank, a natural 'nek' being best, but where such is not to be found, the overflow may be formed by making a cutting, or by leaving a sufficient opening at the end of the bank. In the event of the latter plan being adopted, the bottom, and side next the embankment, should be protected from scour by substantial stone pitching. A training wall or bank must also be made to lead the water clear away from the outside slope, and protected with stones or bushes where necessary.

Front slopes should always be pitched, if there are suitable stones to be procured in the neighbourhood, to prevent damage from burrowing of crabs and trampling of cattle.

From the foregoing remarks it is not to be understood that all the dams in the country are badly made, far from it, for there are many excellent ones, which have stood firm for years, proving a source of wealth to their owners, and showing them that substantial work pays the extra outlay in the long run. But althou h some grave errors have been pointed out in the general construction of dams, the farmers are not altogether to be blamed, for they have to contend with many difficulties and troubles which most people in the large towns know little or nothing of. What they have chiefly to struggle against are

the terrible droughts which are so frequent in the back country, and which sometimes last for years, reducing well-to-do farmers to a state of bankruptcy. Often and often are they obliged to shut up their houses, collect what stock is left to them, and trek with their wives and families to distant parts of the country in search of veldt and water, not knowing when they again can return to their homes.

How then can it be expected that people, living in this hand-to-mouth manner, are able to pay for expensive surveys and construct proper and substantial works ? It is entirely out of their power, and the most they can do is to construct, with what labour they have about the farm, a dam to hold as much water as possible, adding to it as time and opportunity permit. Often a heavy storm destroys all their labour, before the work is properly finished.

An Irrigation Act was passed a few years ago, authorising the loan of money, on easy terms, to enable farmers to construct dams, and other works, for irrigation purposes, and many took advantage of it, but a great deal has yet to be done to give effectual assistance to the poorer class of farmers.

The Dutch farmer, as a rule, adheres tenaciously to the methods used by his forefathers, in dam making and farming, but many are now appreciating the use of such scientific appliances as siphons, turbines, and pulsometer or other pumps for irrigation.

When he sees lands which are regularly irrigated, thriving and producing all kinds of crops and grain, whilst his own are dried and parched with drought, he naturally wishes to possess the like, and I have no doubt that, in course of time, large tracts of fine land which now only afford a precarious support to a few sheep and goats will be made to produce an abundant supply of grain and other produce, the most of which, at the present time, has to be imported at a great cost.

Those who have travelled much in the Karroo cannot have failed to observe that wherever a farmer has been able to irrigate, the luxuriant vegetation appears to the eye, wearied by miles and miles of dry and stunted bushes and with nothing to vary the monotonous appearance of the country, to be a veritable oasis in the desert. There is no reason why these patches of cultivation should not be more numerous and extensive than they are at present, for on most farms there are sites more or less favourable for storing the rain which falls so seldom, and which should in consequence be preserved as much as possible, to meet the demands in time of drought.

Many of these sites are on Government lands, and the one at Van Wyk's Vley is one of the most extensive and favourable yet brought to the notice of the public. A short description will now be given of the manner in which the works at that place were carried out. The designs were prepared by Mr. John G. Gamble, the Hydraulic Engineer to the Colony. The contract for the work was signed on the 28th October, 1882, and the first ground was broken on the 1st December following, the author having been appointed resident engineer.

Before the contract was given out for tender, trial pits were sunk along the centre line of the bank in order to find, if possible, an impermeable stratum on which to found the puddle core. These were put down by the foreman (R. Taylor) who came out from England to undertake borings. He found on the one side of the kolk or river a stratum of hard rock, varying from 8 feet to 17 feet below the surface of the ground. Close to the east side of the river no rock could be found, and it was supposed to have been cut off in or near to the other side, this was afterwards, as the work proceeded, found to be correct. Instead of rock, a succession of shales was found, overlaid on the koppies, which formed the abutments of the bank, by beds of a very hard rock, which are much split up but cemented together by deposits of lime. A bore hole was put down on the east side by Taylor to a depth of 50 feet but he did not get through the shale. These shales for a depth of 10 or 12 feet were of a very friable and porous nature, and it was found necessary to excavate to a considerable depth to get a good foundation.

The contractors (Messrs. Gillet & Perez) then began the work by clearing the ground of all stones, sand and bushes, and by loosening it with picks and ploughs to a depth of 8 inches. On this prepared foundation the embankment was raised. Simultaneously with the starting of the bank, a trench was excavated, extending the entire length of the bank, and down to the rock where rock could be found. This trench was 8 feet wide at the bottom, the sides having a batter to meet the puddle core of the bank, at the natural surface of the ground. It was then filled in with puddled clay, made as will afterwards be described. The puddle core or wall, placed in the centre of the bank was carried up to within 3 feet of the top, where it reached its minimum width of 4 feet, its sides having a batter of 1 in 5.

Great difference of opinion exists amongst engineers as to the proper position of puddle in earthen dams. Some advocate the

placing of a layer on the face of the inner slope, immediately below the pitching. This method can certainly not do much harm, but it is doubtful if it does much good, at all events for banks of any great height, for in this hot climate, when the water in the reservoir is low the puddle is liable to crack and this materially affects its usefulness.

Another plan is to construct a puddle wall in the centre of the bank, continuing it down till the rock or an impermeable strata is reached. This was the method adopted here. The use of this puddle wall is to prevent as far as possible a leakage of water through the bank, which might prove dangerous to its safety, and which is generally caused by bad workmanship, use of permeable material or the burrowing of crabs and mice.

Most people know what the meaning of puddle is, but for the benefit of those who do not, the following description may be useful. The material used is clay, freed from roots, stones and dirt. When made by hand, it is mixed with water, cut up and worked with spades, well tramped and kneaded until it is of the consistency of putty. It was made in this manner for Van Wyk's Vley reservoir. If the clay is pure, as much sand may be mixed with it as is consistent with its holding water, for if there be too little sand it is liable to crack in dry weather. This would more particularly apply to puddle laid on the slope than to that in the centre of a bank. If too much sand is used the puddle will become porous and therefore useless for the purpose for which it is required.

After the puddle was made, it was brought on to the bank in carts drawn by mules, which were found to be much better than bullocks or donkeys for this part of the work, and then spread in horizontal layers about 9 inches thick, well beaten down to the preceding layer, which was first thoroughly wetted, so that the layers would amalgamate together.

Each layer as far as possible was carried the entire length of the bank at once, but where this could not be done steps, of a thickness of one layer, were left so that a proper junction could easily be made.

The forming of the bank and the puddle core were carried on simultaneously in the following manner. When a layer of puddle was finished, it was allowed to dry till it was sufficiently stiff to bear the pressure of the earth placed alongside it. This was brought on in mule carts, while the bullock and donkey carts were worked towards the outside of the bank. This way of working was necessary because bullocks and donkeys cannot be guided so well as mules or horses, and

when once on to the puddle, they sank for several feet, and it was then a matter of great difficulty and loss of time to extricate them.

The earthwork of the embankment was made in layers of about 1 foot thick and well beaten down with heavy rammers before the next was laid on it. The best material was selected and placed to form the water side of the bank, extending back to the puddle core. No stones or shale were allowed on this part of the work, but could be placed behind the core of the bank, due care being taken that there was sufficient fine material to make the mass solid.

The embankment was formed in concave layers, that is to say, the bank was kept from 2 to 3 feet higher on the outside, sloping downwards to the puddle core. This method makes the bank more solid and tends to prevent slips, and should always be adopted, the extra trouble being more than compensated for by having a stronger bank, with the same material.

The contractor, not being a man to lose time or waste opportunities, took advantage of moon-light nights to bring on the earthwork. This was entirely confined to men who owned one or two spans of bullocks, one span working during the day, and the other at night. He paid for the work done at so much per load, brought on to the bank, a certain number of loads being fixed as a minimum, for men had to be employed in spreading and ramming the ground. In order to keep tally of the number of loads, each driver received a ticket from the man in charge when he tipped his load, and when the number of tickets reached ten, they were exchanged for another representing ten, and in the same way when these reached ten, they were given up for one worth 100. These tickets were presented at the contractor's office at the end of the week and paid for according to the rate per load. This system worked well and a large amount of work was thus done. When the bank had been raised to a convenient height levelled pegs were put in, and the front and back slopes trimmed to their respective batters of 3 to 1 and 2 to 1. The front slope was then covered to a thickness of 18 inches, with a layer of stones and small boulders, on which was placed the pitching of stones 12 inches thick.

To guide the men in laying the pitching properly, pegs were placed at intervals of about 30 feet along the bank, and about 20 feet apart down the slope, on which stout cords were stretched, thus giving the proper level and slope. When the pitching was finished a layer of small stones or gravel was spread over it and raked about to fill in the interstices between the stones.

As all made-banks are liable to settle down more or less, the top as finished was kept higher than the normal level, by one half-inch for every foot of vertical height.

A coating of 3 inches of gravel was spread over the top of the bank to protect it from rain, and the trampling of cattle and sheep; and along the edge of the inside slope a dry stone wall 2 feet wide by 2 feet high was built as a protection from spray, &c.

The water is led from the reservoir to the main furrow, by means of two iron pipes, one of 20 inches in diameter and the other 12 inches. These pipes pass through a culvert made of concrete 4 feet wide by 6 feet high, which is built in the solid ground in the kopje which forms the east abutment of the embankment. At the reservoir end of the culvert is built a stone tower, in cement mortar, 33 feet high, with an external diameter at the base of 13 feet, and at the top 10 feet; the internal diameter being 5 feet. This tower is for the purpose of working the different valves, of which there are five in all, each of the two pipes being provided with two, a shuttle valve on the outside, and an ordinary one in the inside of the tower.

The fifth valve is a 6-inch one to be used for any water which may collect in the tower. Access is obtained to the top and inside of the tower, for working the valves, and for making repairs, by means of iron steps built into the masonry.

The top of the tower is covered with cast-iron plates, resting on the walls and on the channel irons, which support the pillars through which the valve rods pass.

The 20-inch pipe passes through the base of the tower, its centre being nearly 26 feet below the highest water level. It is then laid on the floor of the culvert, extending as far as the plug wall, or as far as the puddle core of the embankment.

The 12-inch pipe draws water from the reservoir at a point 13½ feet below the highest water level, passes down through the tower into the culvert, where it rests on brackets supported on short iron columns, at a height of 3½ feet above the floor.

At intervals of 9 feet along the top of the culvert, masonry rings, set in cement mortar, 2 feet wide and 1 foot thick, were built to arrest as much as possible any percolation of water, and so that the earthwork and puddle of the bank should join firmly with the masonry. The filling in of the space between the top of the arch and the surface of the ground was carefully done. A layer of ground was first put in and thoroughly soaked with water, well trampled and

rammed till all the interstices between the culvert and the excavation above the springing of the arch were filled in. After the pipes and valves were properly fixed, the portion of the culvert from the tower for a distance of 75 feet was built in solid, the material used being principally concrete with a little masonry. This plug was made longer than is usual, on account of the shaly nature of the ground below the culvert. The depth of this shale, as I before mentioned is unknown, a 50-feet bore having failed to pierce it. It is not unlikely that the water from the reservoir will soak through this shale and issue out at some point or points beyond the bank.

Advantage has been taken of a nek some distance from the bank to construct the overflow. The ground was excavated and levelled for a width of 150 feet, which is calculated to be amply sufficient to carry off the surplus water.

When the water in the dam is on a level with the bottom of the 20-inch pipe, that is, is empty, the area of what remains in the kolk is 34 acres. This can only be used for watering stock. The extent of land covered by water when the dam is 4 feet deep at the tower, is 500 acres, and at present (March 1886), with a depth at tower of $6\frac{1}{2}$ feet, it is at least 800 acres.*

Beacons have been erected at suitable points outside the dam so that its area can be easily and expeditiously measured at different levels. This will subsequently be of great value, as a close approximation can be made as to the amount of water available for irrigation and other purposes.

Careful and regular measurements have been taken of the depths of the water in the dam to find out, as far as possible, the rate of evaporation and soakage.

No water at all was taken from the dam in 1884, so the amount as shown in the annexed table represents exactly what was lost by evaporation and soakage during that period.

Irrigation was first commenced in July 1885, and has been continued up to the present time.

The amount of land under cultivation is about 25 acres, but the water used for this patch bears a very small proportion to that lost by evaporation. In a whole day's leading, no appreciable difference in level of reservoir could be detected, the loss during the year 1885 may be considered as almost entirely from evaporation and percolation. The observations of the two years can thus be compared, from which

* No chance to measure yet.

will be seen that the holding properties of the dam have much improved. In 1884 with a mean maximum temperature of 78°·8, the loss of water for the whole year was 89·6 inches, whilst in 1885 with a mean temperature of 80°·1, or a daily increase of 1°·3 the loss of water was 76·1 inches, or less by 13½ inches than that of the previous year. This is satisfactory, as it shows that, so far, percolation has decreased.

There is a rain gauge fixed a few hundred yards from the embankment, but it is necessary for estimating the amount of water that might be expected from a certain fall, to have stations fixed at several points of the drainage area of the dam. Such other stations have been established recently. The rains being very partial, a storm may occur only a few miles away and bring a considerable quantity of water to the dam, while the gauge at the bank registers nothing. Judging from the observations that have already been taken here, one half-inch of rain practically gives no increase to the dam, unless it immediately follows a previous fall. (See annexed report on the rainfall, 18th May 1885.)

In conclusion may be given a short description of the distributing channel or furrow. Mr. Alston's tender being accepted, operations for this part of the work were commenced on the 1st of January 1885, and finished within the specified time of five months.

The dimensions of this channel are 3-feet wide at the bottom, 11-feet wide at the maximum water level and 2-feet deep, giving a sectional water area of 14-square feet.

Banks were made on either side 2-feet wide on the top with a mininimum height of 3-feet above the bottom of the furrow.

The slopes were 2 to 1 below, and 1½ to 1 above the highest water level. Wooden forms were constructed to the finished section of the channel as a guide to the workmen.

The gradients of the channel are at the rate of 3-feet per mile for the first half mile, 2-feet per mile for the next two miles, the remaining length being at the rate of 1 foot 9 inches per mile.

The bottom and sides for the first quarter of a mile are pitched with stones, but the remaining part has no such protection, and up to the present time has stood well, the water flowing regularly, without scour. Openings, protected by pitching, have been left in the banks at suitable places, to carry off flood water ; these will probably require some repairs after storms.

The branch main road to Kenhardt, *via* Van Wyk's Vley, is carried

across the channel by means of a bridge, with stone abutments and wooden beams, the main road crosses by means of a drift pitched with large stones, and well gravelled.

The length of the channel is a little over four-and-a-half miles, the surveyed plots beginning at a distance of about two miles from the reservoir. The land along the first portion of the furrow is at present reserved by Government, for tree planting &c., it being too narrow to be given out as corn lands.

A variety of trees and slips were planted here last July and August and most of them are thriving. Some poplar slips have now attained (in nine months) a height of more than 8-feet, showing that they at least grow exceedingly well. Mr. Alston has charge of the tree planting here, and will test what trees are most suitable to this part of the country, and the result, being a matter of great public importance, will be looked forward to with interest both by those who are lovers of nature and by those who look to the financial side of the question.

THE "!NARAS." ACANTHOSIOYOS HORRIDA HOOK.

By R. Marloth, Ph.D., M.A.

[Read October 27th, 1886.]

Amongst the large number of curious plants which Southern Africa has produced are two most remarkable ones, which occur in the neighbourhood of Walfish Bay. The one, *Welwitschia mirabilis*, the "Tumboa" of the natives, was discovered some twenty-five years ago by Welwitsch. The other plant, the first specimens of which were brought to Europe by the same traveller, is our present subject, the Naras, or more correctly pronounced ! Naras,* *Acanthosioyos horrida*. Welwitsch found both plants much more north than Walfish Bay, a little south of Mossamedes, and realizing at once the scientific value of his discovery with respect to the first plant, he took care to collect a good number of complete specimens.

With the Naras, however, he must have been less lucky in his findings. There are only incomplete specimens in his collections and the description of the plant, published in Oliver's "Flora of Central Africa" does therefore not mention some important characteristics of it. I shall spare you, of course, the botanical diagnosis of the plant, and deal here only with its external habit and its value to the natives in the neighbourhood of Walfish Bay.†

Besides a few spots near to the Capes Cross and Frio, the Naras grows almost exclusively in the angle formed by the lower Kuisib and the coast. There is little doubt that this river formerly entered the sea at Sandwich harbour, but the advancing sand dunes barred its way and compelled its waters to flow more northward, whenever it happened that flowing water came down so far; an event sometimes unknown for more than twenty-five years. Although there is very rarely *open* water in the lower course of the river, its sandy bed contains always the precious liquid some depth under the surface, and a portion of this water forces its way underneath the sand dunes

* The ! indicates the palatal click of the Namaqua language.

† For fuller information see the author's monograph of the plant in *Engler*, Botanische Jahrbücher, Vol. IX.

down to the coast. This is the source of the splendid water found close to the shore at Sandwich Harbour and then again at Sandfontein, at the back of Walfish Bay, and this subterranean water enables also our plant to grow on the sand hills between these two places.

No other vegetation can exist on this moving sand under a tropical sun and a continuous drought, for rain is almost as rare there as here for instance an earthquake. The root of the Naras descends deep into the sand until it reaches the hidden moisture. It has sometimes a length of forty feet. A French missionary, Rev. Duparquet, who calls Damaraland *Western Caffraria*, and whose statements are published by Monsieur Naudin, and taken thence into a recent number of the "Gardener's Chronicle," tells us that the root is occasionally about 350 feet long and descends to unknown depth. Well, the Naras is a curious plant, but a root of 350 feet length and an unknown depth into the bargain is rather a strain on a naturalist's imagination.

The Naras is a cucurbitaceous plant. It belongs therefore to a family, some members of which are very useful to man. I need mention only the pumpkin, cucumber, watermelon and rolaquint. But its appearance is totally different from all its relatives.

The root puts forth successively several shoots of a finger's thickness. But no leaves are to be seen thereon. The shoots branch out several times and bear two conical spines at each node. As the very slender branches are much entangled and interwoven they cover the sand dunes like a hedge and it is very unpleasant to pass through such a hedge on account of these sharp pointed thorns.

The prevailing wind on this part of the coast being a southerly one, the sand dunes change almost daily their form and bury consequently very often the Naras bushes; but that does not matter. The more the sand tries to cover it, the longer become its shoots—and always provided that the root has reached the moist region of the sand—the plant remains victorious and always afresh overtops the sand.

It is a most striking sight, these green bushes on the top of the ever-moving hills, whilst the sand around them is bare and nude, and it has been a puzzle to a good many people why this plant almost always prefers the exposed tops to the valleys between. The truth is the plant does not favour at all these spots, but being attacked by the sand it struggles for existence and whilst the sand is heaped around it and on it, it grows longer and longer, and it is strong enough to succeed in a struggle which would overwhelm any other plant.

It is wonderfully adapted to such an existence. All the kindred plants are conspicuous by their largely developed leaves, but this member of the family does not form any leaves at all, because it would be impossible for the plant to supply the necessary quantity of water for their maintenance and the leaves would be quickly scorched. The evergreen shoots and twigs do the work of the leaves, the respiration and assimilation. But even these require water for their life. The plant must therefore possess a well-arranged system of water-pipes. The piece of root, which you see here, is as light as cork, almost spongy. The reason is, that it contains numerous and ample vessels, visible to the naked eye. In this dry piece, these vessels are filled with air, but in the living plant they contain water, which is carried along them from the rootlets deep down below to the green parts of the plant on the tops of the hill.

There are also some excellent arrangements in the anatomical construction of the younger parts, whereby the green tissue is protected against the scorching effects of the heat, but it would lead us too far to enter here into this question.*

The flowering season begins with the summer, the first fruits ripening about Christmas. As the plant is dioecious, not all the bushes bear fruit, on the contrary, the female plants seemed to be in minority. The fruit attains the size of a child's head ; it is green even when ripe ; is armed with sharp knobs and becomes soft on ripening. The natives use a bone-knife, made from the rib of an ox for opening the fruit, because the juice of it would bring about an inflammation of the fingers. Differing from other cucurbitaceous fruits, its six parts are easily separable from each other like the sections of an orange. The colour is that of a carrot, flavour and taste something like spanspeck, but rather sharp.

With the ripening of the Naras pleasure and joy enter the hearts of the natives who live between the sand dunes. They are a tribe of Hottentots called "Topnars." Their existence in such a desert may appear to us a very wretched life, but to them it is then really a paradise, for in the commencement of the Naras season they do nothing but pluck the fruit, lie on the sand all the day long and devour as much of the "lekker kost" as their stomachs will hold. Evening and night are spent in singing and dancing, and in the morning begins again the hard work of the previous day.

As the fruits become more numerous, those of the nation who are

* See figures in above monograph.

wise begin to make preserves. They put the pulp in a pot and boil it down to a thick pap, pouring it then through a sieve of their own invention, which retains all the seeds, whilst the pap hardens on the sand to a flat cake. Both these products, the seeds as well as the inspissated pulp, are put aside for later times, when no more fresh fruits are to be met with. They form the principal food of these people, who eat the seeds as they are and boil the pulp up simply with water, obtaining thus a very nutritious soup. The seeds are also regularly brought to the Cape Town market, known here as butterpits. They contain much oil, whilst the pulp is rich in albuminous matter and sugar.

The most interesting property of the fresh fruit is its effect on milk. During my travels I heard it mentioned from different natives that the aroma of the Naras acts on milk like rennet. They stated invariably, that milk kept for a few hours in a room, where a Naras lies, will turn on being boiled. When I came back to Walfish Bay, I inquired there into this question. All the Europeans confirmed the statements of the natives. One lady mentioned for a proof thereof, that she placed a jug with milk in a cupboard where a Naras was and found that the milk turned on being heated a few hours afterwards. On my question, whether she had kept a portion of the same milk somewhere else and tried it for comparison after the same time, she answered, no ; admitting then, that her one-sided experiment was not conclusive.

At my return to the bay the Naras season had already passed several months ago, but as it was of the utmost interest to me to ascertain how much of these strange statements was true, I asked some men to look out for a few fruits. On hearing that I wanted the Naras only for experimenting with them on milk, they laughed apparently at my ignorance in a matter which was known to every child in the country, but by the promise of some coffee and tobacco I induced a small party of men to search the whole area for full grown Naras. They brought me three, but they were hard. Yet, by nursing them properly, that means to say, by exposing them to the sun at day-time and taking them into the room before the evening mist came on, I managed to ripen them in a week's time. Fortunately I had a spirit-lamp and some test-tubes with me, which enabled me to make a series of experiments.

I think it unneccessary to enter here into the details of these experiments and I shall mention only the results of them.

The juice of the Naras contains really a principle which acts on milk like the rennet of the calf.

This "vegetable rennet," as I may call it, is *not* volatile. It is therefore not the aroma of the Naras which spoils the milk at Walfish Bay, but the negligence of the milk-boys, who eat Naras all the day long, being certainly not over-cleanly and careful with their hands and milk-pails. As there is no grass near to the bay, the cows are kept at a place about seven miles off. The boys bring the milk from there every morning, and it is a general belief that milk, brought by a boy who carried at the same time a Naras, even in his bag only, will turn.

My experiments showed that there is no such active principle in the aroma of the fruit, but as the coagulating power of the Naras-juice is nearly 1 in 2,000, that means to say, that one drop of it is sufficient to coagulate three ounces of milk, or a tea-spoonful for one gallon and half, we can well understand how easily the peace of a Walfish Bay kitchen is endangered during the Naras season.

Concluding my little paper I may be allowed to draw your attention to the two most remarkable facts connected with this plant.

The first is the mere existence of it under such trying circumstances. And it exists not only itself on these moving sand-hills, but it renders there possible the existence of a whole tribe of natives, whom it furnishes with food and drink.

The second fact is the renewed proof that the man of science can never rely upon the statements of others, for although in the country of the Naras nobody doubts the fabulous quality of its aroma, this universal belief is inconsistent with the result of exact experiments.

THE ORBIT OF COMET 1886 (FINLAY).

By W. H. Finlay, M.A., F.R.A.S.

[Read 24th November, 1886.]

I discovered this comet on the evening of September 26. After computing several parabolic orbits which failed to satisfy further observations, I calculated the following elliptic elements from my observations on September 29, October 10 and October 21.

T	= Nov. 21^{d}·6056			Greenwich mean time.
π	10°	46′	18″	Ecliptic and mean Equinon 1886·0.
Ω	53	38	14	
ι	2	57	0	
log a	0·485900			
log e	9·835091			
μ	662″·456			
Period	1956 days.			

It is not improbable that the Comet is identical with the first of 1844, the "lost de-Vico."

THE STORMS OF SOUTH AFRICA.

By A. G. Howard.

[Read 4th May, 1887.]

INTRODUCTION.

The subject which I am going to deal with to-night, is one which has exercised the minds of meteorologists ever since meteorology has been in existence; that is to account for the various phenomena relative to storms.

Ever since the invention of the Barometer, theories have been advanced on the subject of its oscillations.

First, it was thought that the wind was the cause and not the effect. But no reason was shewn why the wind blowing from one direction should cause the mercury to rise, and from another direction cause it to fall.

A great light was thrown upon the question when simultaneous observations were plotted down, and lines of equal pressure, called isobars, drawn. It was then discovered that these isobars invariably resolved themselves into a form approaching to that of a circle.

The next discovery made was that the wind always blew round the circle with the lowest pressure to the left, thus blowing from opposite directions when the centre was an area of low or of high pressure.

This was the Cyclonic Theory.

According to this theory, every storm consists of a circular-shaped area of low pressure, round which the wind blew in a direction against watch hands in the Northern Hemisphere, and with watch hands in the Southern one. But here the whole truth had not been arrived at, and many accidents were caused by a sudden chopping of wind, quite at variance with the cyclonic theory.

Now although the greater number of storms are not entirely cyclonic, yet, as a matter of fact, some storms are true cyclones, and are experienced in the West and East Indies, as well as often being found, of small extent, rolling along between two opposing anti-cyclones.

The word anti-cyclone I do not like at all, as the area of high pressure so called is caused by an advancing current which is opposed by another one, as will be seen in the body of the paper. I prefer the term "high pressure area," which does not give any inference beyond assuming that there must be "low pressure" ones, which is true, while the term "anti-cyclone" infers the existence of prevailing "cyclones," while the truth is, that most of our storms are not entirely cyclonic.

Another theory which has been put forward, is the centripetal one. In this, the area of lowest pressure is assumed to be very small, and the winds to blow straight towards this point from all sides, the only modification being due to the earth's revolution. Thus the very large storms which cross the Indian Ocean would have the winds blowing spirally towards the centre, while the smaller tornadoes of North America would not shew this spiral development at all.

Here again the whole truth is not grasped. Spiral winds do blow, but they are not general. In tornadoes the wind, I believe, does blow straight to the centre, but on this point I cannot speak with any degree of certainty.

Another theory is to consider the area of lowest pressure as almost a line, the winds blowing straight towards it from all points. This theory is true only for equatorial winds ; for polar winds it is utterly at fault.

I have tried to set forth in the following paper a theory based upon the siftings of all these theories.

In investigating the question of Storms in South Africa, I have tried all the theories, but find each one unsatisfactory. I have thus, upon due consideration, placed the whole of them on one side, and endeavoured, from actual observation and deduction, to arrive at a true understanding of this question, using the deserted theories as stepping stones to reach the result which I have tried to make clear in the following pages.

THE STORMS OF SOUTH AFRICA.

I wish, before entering into the question of the nature and origin of Storms, to have it distinctly understood what is meant by the term "storm." A storm is nothing more than an atmospheric movement tending to restore a ruptured equilibrium. Every loss of equilibrium, of however slight a nature, generates a storm in the strict sense of the word, although the winds evolved may be only slight. I say advisedly, an atmospheric movement, although I am aware many will

disagree with me. They will point out numerous winds blowing, where no storm is, as they assume, notably the trade winds. But I will answer them, that no wind whatever can blow without a disturbance of equilibrium, and that the trade winds blow constantly in one direction because of the existence of a permanent storm along the equator, the so-called belt of calms being in fact the line of constant disturbance.

To understand the nature of storms correctly we must study the distribution of the atmospheric pressure generally over the whole earth. First, let us assume the earth at rest and no disturbing influence whatever at work; what state would the atmosphere be in? The whole atmosphere would be in a state of perfect calm. Now, let us assume the sun to shine vertically over one spot on the earth, what would ensue? In this case the air would rise perpendicularly at that one spot, and a lower current would set in towards it, while a higher one would flow from it, for a limited distance around that spot. And now let us assume that the earth turns on its axis, the sun being vertical over the equator. Here the sun's effect would travel completely round the earth, the result being a belt of rising air, with a lower indraught and upper overflow for a limited distance on either side. We will confine ourselves entirely to the atmospheric movements in the southern hemisphere. Before the upper current had reached the surface of the earth, a great quantity of air would have been drawn from the south towards the equator, the indraw extending almost to the pole. Consequently there would then be a vacancy to fill, and the upper current on reaching the ground would continue its course towards the pole, to fill this vacancy, till meeting the cold banks of high pressure round the pole, it would momentarily force it back; but as the polar pressure increased, this current would in its turn drive the equatorial one back towards the equator, until having penetrated further than necessary to restore equilibrium, a return oscillation would take place and the polar current recede.

We thus see that the atmosphere would be divided into three low pressure belts and four high pressure ones, that is to say,—a low pressure belt round the earth at the equator; two high pressure ones over the north and south tropics; and two low pressure ones in the temperate zones, while two high pressure areas would rest over the poles. Every outburst of solar energy would deepen the equatorial belt, and cause the air to be drawn in greater quantities from either

side, while the upper current being intensified would rush over the dense bank of high pressure, and down through the temperate zone to the pole, displacing the polar current, but by that very act giving strength to this polar high pressure area and causing a return oscillation.

Now let us note the effect of the inclination of the earth's axis, whereby the sun appears to travel during six months over a belt of 47 degrees of latitude, that is, from $23\frac{1}{2}^\circ$ N. to $23\frac{1}{2}^\circ$ S. Owing to this apparent motion of the sun, as the disturbed portion is always, in the first instance, where the sun is vertical, the belts of high and low pressure are constantly oscillating north and south, one complete oscillation taking twelve months to perform. Thus, Cape Colony, when the sun is at its southern limit, has the south tropical belt of high pressure to the south of it; but when the sun is at its northern limit, this belt is far to the north of us, and we are on the northern edge of the temperate-zone area of low pressure. Were it not for the unequal distribution of land and sea, the arrangement of atmospheric pressure would be regular, and as sketched above, but as it is, it is far from being so, although on the whole, it is as I have described it, which is near enough for the purpose of investigations where Cape Colony alone is concerned.

By taking the mean barometer readings for different months, and extending over a number of years, isobaric charts have been compiled, and these shew very clearly the distribution of high and low pressure. By referring to one of these for January, it will be seen that a large bank of high pressure lies to the south-west of us, and another to the south-east, being joined together by a col or neck of comparatively lower pressure. The centre line of this area is the limit of the ordinary influence of the equatorial indraw, but when great disturbances take place, the indraw extends further south, and the upper current becomes intensified, bringing violent south-easters over the Colony, and storms in the area of low pressure to the south of us, which after a definite period, return from the south on to the Colony, with north-west winds and rain.

To the north of the Colony, and stretching well to the interior of Africa, is a wild Karoo, hot and dry, called the Kalahari Desert, with extensions to the West Coast and down to Namaqualand. This hot plain plays an important part in our Colonial meteorology. The constant heat generates a constant upward flow of air, and a consequent surrounding circulation. Thus over the Orange Free State and at Kimberley the wind flowing round this area is north or

north-east. A portion of the south-east trade flowing from the high pressure area over the South Indian Ocean is deflected and drawn towards this area of low pressure, and intensifies the north wind which flows on to the Colony. How far the geographical distribution of mountains and plains affect this deflection is an open question, but I have no doubt as important a part is played by these in South Africa as in other places.

When any great disturbance takes place at the equator, a strong current sets in off the tropical bank, towards the point of disturbance, and the portion which mingles with the northerly current before-mentioned, increases its intensity considerably, and forces it further on to the Colony. Here it meets the south-east current of the trades, and a semi-cyclonic movement is generated, but as the pressure of the advancing and opposing currents is greater than the lateral pressure, the area of disturbance formed will be of an oval shape, an ellipse in fact, the minor axis being along the point of greatest pressure. Various modifications of this form are experienced when the advancing and opposing currents do not meet at an angle of 180° but at a smaller one.

The north wind, having passed over a tropical portion of the earth, is, as a rule, warmer than the south-east one, consequently it flows up and over the latter, while the south-east wind is deflected and flows more easterly, until reaching the edge of the high pressure area to the south-west of us, the combined forces of the winds increase their power to a very great extent.

So that when an area of low pressure extends across the Colony the winds will flow direct in to it from the north-east, north and north-west, but will flow round it on the south and west sides, as east and south-east winds.

As the power of the north current increases, it forces its way on to the Colony, the area of low pressure thus advancing, until the constant overflow above on to the area supplying the south wind, and its consequent banking up, causes a return oscillation. At the time when the two currents are stationary (which sometimes lasts for days, when the currents are balanced in intensity) the winds blow strongest, and often independent cyclones form in the low pressure area between them and roll off to the east. This I have noticed over and over again. When the south current has gained the victory, it gradually pushes the north one back again, but as this south current generally comes from the south-west, in the form

of a high pressure wedge, the elongated area of low pressure over the Colony is split in two, one portion of which recedes to the north, and the other as a cyclone rolls off to the east, bringing storms to Natal and the Eastern Province.

Sometimes the northern current flows right across the Colony, the area of low pressure being driven down to the south-east of us. Then if the south current sets in the two will coalesce, and a very high so-called anti-cyclone be the result. I need scarcely explain that when two currents, flowing in comparatively the same direction, coalesce, the pressure is increased without any cyclonic movement, but when the two currents are opposed, a low pressure area is the result, and often independent cyclones are formed.

When the upper north-west equatorial current is intense it flows down into the area of low pressure to the south of us with great force, and meeting the polar current, drives it back, a storm being the result, the north-west wind blowing straight to the lowest pressure, and the south-east winds being cyclonic. As soon as this current has expended its force, and the polar current becomes banked up and increased in intensity, a return oscillation (as before described) takes place, the distance to which this return storm pushes to the north being proportionate to the intensity of the north-west upper current in the first place. These storms come on to the Colony from the south-west, and even west, and are preceded by easterly and north-easterly winds of no very great intensity. As the area of low pressure advances the wind becomes more northerly, till when the barometer is at its lowest, the wind is generally north-west, but goes round very smartly to the west. This change is usually accompanied by heavy squalls and rain.

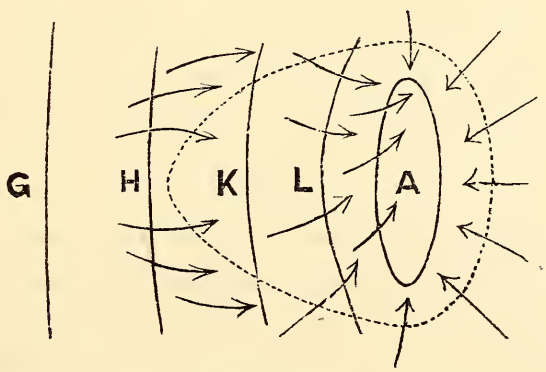
To more fully explain the nature of storms, and how they are generated by two opposing currents, I have prepared some diagrams, which I will now refer to.

Fig. 1 shews a section through a storm on the plane of the line of motion.

(Fig 1.)

E is the equatorial current and D the polar one. When the two currents come into contact with one another, as E is a rising wind and D a falling one, the rising one will naturally flow up and over the falling one, the line of meeting being shewn at A B. Both currents flow in almost parallel lines at different heights, one having a downward and the other an upward tendency, and wherever either current meets the line A B, the particles of air are carried upwards towards B. At F they reach a point of equilibrium, but being forced upwards by the particles below them, they flow outwards on either side and compensate for the loss sustained by the lower currents.

Now it is evident that if there is an increase of atmosphere at N it will press unduly on D and the whole system will move towards the south. A vacancy will be caused at A, by the upward current from A to B, and the surrounding surface winds will rush in to compensate for the loss, the general direction of the winds being as shewn on Fig. 2.



(Fig. 2.)

Suppose the initial banking up of E (Fig. 1), which caused the motion, to cease; as the motion is now towards the south, a point would be ultimately reached where the weights at D and N would be balanced.

But the depression will not stop here. Dynamic force has to be overcome, and so the disturbance will still travel to the south, till the increased banking up of D will not allow of its further motion, and it will consequently come to a standstill.

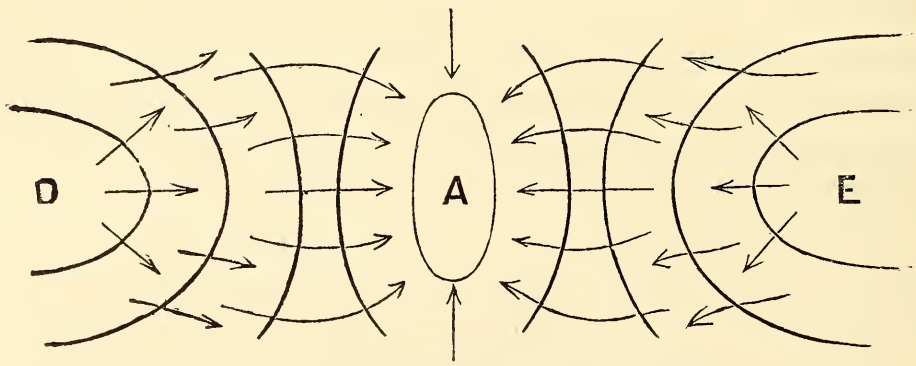
But the greatest weight of air being now at D, the whole system will be forced back and be pushed against E in a similar manner, to be in its turn beaten back to D, and so the oscillation will be kept up till equilibrium is restored.

Sometimes the two currents will bank up simultaneously. The depression will then be motionless, but the winds will be very strong

and the barometer oscillate violently. Inequalities of surface contour, or patches of intensely heated soil will be enough to generate independent cyclones, which, once formed, will roll along between the two currents and pass away to the east. These little storms are very annoying in upsetting all the pre-arrangements of the forecaster, and very often make him look foolish before the world.

I have observed the two opposing currents remain stationary for a week over the Colony, and cyclone after cyclone roll along the intervening area.

On Fig. 3 I have embraced more of the two currents, and the storm in the form of an isobaric chart.



(Fig. 3.)

E is the equatorial current and D the polar one, A being the area of low pressure at the plane of meeting, close to the surface of the ground. It will be noticed that the winds flow off the areas of high pressure D and E, first in straight lines on all sides, which by the revolution of the earth are gradually deflected in a direction opposite to the motion of the hands of a watch on its face. Were there no disturbing element, these winds would become more and more circular; but coming under the influence of the area of low pressure, they are drawn towards it from all sides. Those of the equatorial current E are seen to blow straight in, and those from the polar one D being deflected round the area in a direction similar to watch hands.

I will now refer to Fig. 1 again.

All atmospheric currents as they travel along contain a certain amount of moisture, although this may be in a state of invisibility. It is a well-known fact that the temperature decreases as we ascend into the atmosphere, and also that if atmosphere containing moisture be carried to an altitude where the temperature is lower than the dew point of its original position, the moisture condenses, and clouds form.

Bearing this in mind it will be noticed that, as the air at A is carried up to B, and contains moisture in an invisible state, this moisture will condense and form clouds at that level. As fresh moist air rushes up, fresh clouds will be formed, each one being larger and more complete than the last, till at A the type will be the large cumulus and cumulo-stratus. As the cloud forming moisture is carried up by the ascending current, a portion will be conveyed back by the return overflow above and being precipitated through the atmosphere at N form various sorts of clouds there. According to the amount of moisture carried up, so will the clouds at N accumulate, and ultimately rain will fall. The direction of the clouds will be parallel to G.H.K.L. on Fig. 2. Those at G will be faint horizontal streaks of cirro-stratus, sometimes barely visible to the eye. Following these at H will come nodules of cirro-cumulus. The pinnacles of these nodules penetrating into the great west upper current above the line B.M. (on Fig. 1) will be blown into thin streaks or threads and form the cirro-filum or thread cirrus. Simultaneous with the dispersion of the upper portions of these nodules, a precipitation will take place into the lower current, the rain being blown towards A on Fig. 1. At K and L (Fig. 2) the clouds will be larger and more perfect in form, till at A, the heavy cumulus, cumulo-stratus and nimbus are formed. Beyond A in the equatorial current, hard rounded cumulus, with intermingled stratus merging into cirro-cumulus and high mottled cirrus will be found. No cirro-stratus will be found on this side of the centre. All along the line A.B. (Fig. 1) the clouds form, and are apt to impinge into the upper current. When this is the case, a precipitation is sure to take place, and if the altitude is not too great, will reach the ground, hence the area of precipitation (shewn by the dotted lines on Fig. 2) is always more on the polar side than on the equatorial one. This is why we have more rain, with our winter storms, after the centre has passed than before.

And now let us study for a few minutes the movements of the barometer during the passing of the system. E (Fig. 1) is a warm current and consequently light, while D is a cold one and of course heavy, but if the whole system is moving towards the south, the intensity of E must be great enough to overcome the weight of D, and consequently the pressure at N must be greater than at D. From C to A the heavier current is gradually becoming less, the current E being much lighter, hence the barometer will gradually fall from

C to A, and as the pressure at N is greater than at D, the barometer will increase from A to N.

Hence whichever way the system is travelling, the barometer will fall till A has passed, but there will be a great difference in the manner of the changes, for while the gradient from C to A is gradual, that from N to A is very abrupt.

So that if with a north-east or north wind, the barometer falls very suddenly, we may almost be certain of rain falling when the barometer begins to rise, especially if heavy cumulus or cumulo-stratus clouds are seen to the south-west, from which direction the following winds will blow.

When a depression is advancing on us from the north-west, the first indication of its approach is a sudden rise of the barometer, and the appearance of long horizontal strips of cirro-stratus to the north-west. These strips follow one another and rise higher and higher. Cirro-filum are often seen before this, in long parallel threads from north to south. When the cirro-stratus has reached the zenith, banks of other and lower cirri come up from the north-west, and the barometer begins to fall, the wind then blowing from the east or south-east. As the depression advances, heavier and lower banks of clouds come up till the whole sky is overcast and rain begins to fall. As soon as the barometer begins to rise, which it does suddenly, rain falls in heavy squalls, and the wind blows strong from the north-west. The storm has passed and fine weather follows.

At Cape Town we very seldom have the centre of such a depression pass over us, but when it does, a black south-easter is followed by showers from the north-west.

In summer time the centre generally passes to the north-east of us, so that the winds are easterly as the centre approaches, south as it passes, and west after it has passed. Rain very seldom falls here, but they are the great rain bringers of the East and Natal.

In winter the centre generally passes to the south-west of us, the wind being then from the north-east as the centre approaches, then north and north-west, and finally west, with rain squalls ending in fine weather.

Some of our severest winter storms are of this type. In these the greatest amount of rain falls before the barometer rises.

The great majority of our winter storms are of a different type. In these the polar current is displacing the equatorial one, hence the sudden manner in which they come upon us. The advance wind of a

storm of this type is generally from the north, which changes to the north-west as the centre approaches. As the centre passes, which it does to the south of us, the wind chops round to the west with heavy rain and squalls, which continue as the barometer rises. Fine weather follows with a south-west wind. With these storms the greatest amount of rain falls after the centre has passed.

These depressions bring rain to this end of the Colony during winter and to the other end during summer. Owing to the fact that these storms generally travel alon t he course of the L'Agulhas current, the rain falls in the east with a south wind.

When a depression travels as rapidly as the advance winds blow, a dead calm is very often experienced in front of the storm.

This is very often the case, especially during summer, when the storms come from the north-west.

I have often noticed a gradient between Cape L'Agulhas and Cape Town, which ought to have brought a south-east gale, but only light winds followed, the fact being that the depression, although deep, was travelling as fast towards the east as the advance indraw, in other words it was keeping up with the wind and counteracting its intensity.

I have assumed in all these examples that the two opposing currents are pressing directly against each other; but such is very seldom the case, they generally abut obliquely and sometimes very much so, and then instead of travelling in the direction of the line of the minor axis of the low pressure area, they travel along a line formed by the resultant of the two opposing currents, and the form of the depression changes from that of an ellipse to almost any shape.

And now before concluding, I should like to say a word or two about sun spots, or rather I should say solar storms. It will be remembered that in a former paper of mine, I laid before you the fact, from actual observation, that upon a sun spot reaching the centre of the sun, various known winds blew at regular intervals thereafter; thus from two to three days after, a south-easter was sure to blow during the summer months, while eleven days after, north-west or west winds were experienced.

Now the reason of this is shewn in this paper. As soon as the atmosphere at the equator is affected, an upper current rushes towards the poles. It takes two or three days to reach the latitude of Cape Colony, at which date the south wind blows strongly owing to the atmosphere being banked up to the south of us. The upper current

then follows the sequence before mentioned and comes upon us eleven days after as a south-west depression.

I have not continued my observations on sun spots for the reason that it is impossible for one individual to do everything, and I have found no one interested enough to study this question. Moreover, I do not believe all solar disturbances are visible to us, so that to study this question properly comparison should be made with the observations of magnetical disturbances, which are caused by variations in the sun, and to do this is a study in itself, so that did I devote my spare time to this, I would have to do this alone to the detriment of my other observations, and this I do not care to do.

But if there are any who would like to enter into this investigation, I would suggest the tabulation of magnetical storms, and these I am certain would determine the period of depressions and their nature also. Thus, I have no doubt but that two days after a magnetical storm, a strong north-west upper current flows over Cape Colony, and the barometer banks up to the south, with strong south-east winds. At eleven days after a similar disturbance, a south-west storm of the winter type will come, and so on, regular fixed periods for definite storms. I place the consideration of this question before any one who wishes to further the interests of meteorology.

There is no doubt that electricity plays an important part in our weather. In fact, time may shew that electricity is the one great and sole agent at work, and the sun the great electromotor, the earth being the accumulator.

I do not know whether any present have noticed the manner in which cumulus clouds group, but I have, often. As far as my observations have extended at the Cape, they seem to aggregate in horizontal banks of a definite width and parallel to each other, the line of stratification being along the magnetic meridians. I do not bring forward any reason to account for this, but merely give it as an authenticated observation, leaving it for the future to decide what the meaning may be.

ADOLPH G. HOWARD.

Cape Town,
30th March, 1887.

ON THE ADULTERATION OF BRANDY.

By R. Marloth, Ph.D., M.A.

[Read 29th June, 1887.]

The subject of the adulteration of brandy has been ventilated in the Colonial newspapers from time to time, not only here in Cape Town but also elsewhere. During my stay at Kimberley, about eighteen months ago, I read there similar complaints about the injurious properties of the brandy, to those which I had heard here.

It seemed to be the general opinion that most of the canteen-keepers were dangerous compounders of poisons, who took the brains out of the unsuspecting natives by means of blue-stone, oil of vitriol, tobacco-extract or cayenne pepper.

I thought it therefore worth while to investigate the question and procured four samples of brandy, but having there no analytical apparatus at my disposal, I took the samples with me to Cape Town. This was in the beginning of 1886.

Another journey however, which took me from Cape Town for several months, prevented me from taking up the postponed analysis; and when I returned from the interior, the subject had slipped out of my mind.

It was only about February last, when some remarks in the newspapers reminded me again how desirable it was to ascertain the nature of the virulent poison that was maddening the stalwart Kafir in the streets of Kimberley and the jolly sailor who landed on our shores.

I employed a common workman for the purchase of the samples, which were taken from different canteens of the town. Up to the present I have analysed twenty-four samples of brandy, four from Kimberley, bought there last year, and twenty from Cape Town, bought during the months March, April and May.

I may state at once that none of these samples contained any foreign ingredients detrimental to health. My investigations confirm therefore the results which were obtained at the Excise Department by the examination of forty-seven samples of brandy, and we

may therefrom draw the conclusion, that very little, if any at all, adulteration of brandy takes place in our canteens,—adulteration at least in the restricted sense of the word as we use it out here, alluding only to the addition of health-destroying materials.

Although the brandies which I have analysed did not contain any artificially added poison, I cannot consider them to be a harmless drink. Harmless I mean of course without regard to the effects which the alcohol itself has on the consumer. The majority of the samples contained a considerable amount of fusel-oil, agreeing in this respect with those which have been examined by the Excise-officer.

To prevent any misunderstanding, I may be allowed to consider in greater detail this unpleasant body, the fusel-oil.

The alcoholic fermentation is often described as the splitting up of sugar into alcohol and carbonic acid, effected by the action of some living vegetable cells, commonly called the "yeast." But this is a very rough and incomplete definition of the process, which is not at all such a simple one. On the contrary it is a very complicated matter, the attempt to explain which has given rise to many theories, and which is yet far from being fully understood. We know that whenever such fermentation takes place a considerable number of other chemical substances are formed, 94 per cent. only of the sugar present in the liquid is—as Pasteur has shown—transformed into alcohol and carbonic acid, the other six per cent. supplies the material for the formation of the accompanying substances. The principal by-products are glycerine, succinic acid, some compound ethers, as for instance acetic ether and several other alcohols. In chemical language the name alcohol comprises a whole class of substances which are composed according to a similar formula. The five first members of this series are the methyl, ethyl, propyl, butyl and amyl-alcohols.

Speaking generally however of "alcohol" we always mean the second of the series, the ethyl-alcohol, just as we use the word "salt" in a specific sense, although there are thousands of different salts known to the chemist.

There seems to be a widespread opinion, that the nature of alcohol differs according to the material it is made from. That is a misunderstanding of long established facts. The alcohol is exactly the same in all the fermented and distilled liquors, chemically and physically. It is the same substance that men are eager to produce

in their alcoholic beverages all over the world, whether they ferment the sap of the African palm or the milk of Caucasian mares, whether they distil it from the grain that grows in the valleys of Scotland, from the potato of the German plains, or from the noble grape on the banks of the Charente in the West of France and on the hills of the Paarl in our neighbourhood. It is always this ethyl-alcohol (C_2 H_4 O.) The difference of the various products lies only in the accompanying substances, and as far as the distilled spirits are concerned, only in the bouquet.

What is the bouquet? you will ask. It is a very variable, very delicate and complicated thing, and I can name here only a few of its constituents. You will remember that during the fermentation all the chemical compounds, which I mentioned, are formed. Their relative proportions and absolute quantities vary considerably, according to the fermenting liquid and the character of the fermenting agents, the yeast. Also here cleanliness is of the highest importance. The purer the ferment itself, the purer is also the product. Recent investigations with carefully prepared ferments (yeast) have shown that it is possible to obtain almost pure alcohol, which is not accompanied by any fusel-oil at all.

This portion of the by-products of fermentation, namely the volatile ethers and acids, pass easily over during the beginning of the distillation; the glycerine and the succinic acid, being not volatile, remain of course in the still with the refuse-wine, and the other alcohols with a much higher boiling point than ethyl-alcohol, should equally remain in the still. But they do not, just as water evaporates already much below its boiling point, in fact at all temperatures and even in its solid state, as ice or snow, so these alcohols are volatilized with the vapours of the ethyl-alcohol and water. Their quantity in the products will of course depend on the amount that was present in the original liquid and on the care bestowed upon it during the distillation. The three alcohols with a higher atomic formula than the ethyl-alcohol form principally the compound which is called "fusel-oil." The fusel-oil of the wine is different from that of the grain and this again from that of the potato, but they all contain these alcohols in various proportions.

It is a fact which has long been known, that fusel-oil is highly injurious to health, and especially that of the potato has always been noted as the most objectionable one of the family, but only recent experiments have shown the reason for these facts.

It was Professor Dujardin-Beaumetz in France, who ascertained the effects of these alcohols on dogs. The results of his experiments establish the highly interesting fact, that the higher in the series an alcohol stands the more toxic it is, and the degree of toxicity follows almost in a mathematical ratio the *atomic formula*. He has determined the medium toxic dose for the various alcohols, calculated on the weight of the dog and reduced to a unit in kilogrammes, and he obtained as the result the following series: Ethyl-alcohol, 7·75; propyl 3·75; lentyl, 1·85; amyl, 1·5. You are, of course, aware of the fact that the alcohol contained in brandy or wine or elsewhere is a poison. There is a toxic dose for alcohol just as well as for strychnine. It has happened often enough that a man has been killed by taking too large a dose of spirits. The toxic dose for dogs being 7·75 grammes for each kilogramme of weight, a terrier, weighing for instance five pounds, would require one ounce of ethyl-alcohol to be killed. But in giving him propyl-alcohol, which is the first of the fusel-oil series, half an ounce would produce the same effect, whilst the amyl-alcohol would do it already with 90 drops, being more than five times as toxic as the ethyl-alcohol. The latter is, you remember, the alcohol *par excellence*, the former with five atoms of carbon is *the* fusel-oil "par excellence." It is this alcohol which forms the main portion of the potato and grain fusel oils, and therefore the obnoxious properties of rude potato and grain-spirits are easily understood. The spirit carefully distilled from good wine contains very little of these more toxic alcohols, but in spirits from badly fermented wine their quantity increases, if the distillation is not carried on in such a manner as to prevent their being mixed with the spirit in the receiver.

Mr. Crowe's suggestions in his report to Parliament deserve the full attention of all brandy-distilling farmers, for if the stills are improved in such a way as he recommends a great portion of the fusel-oil will be separated from the brandy.

I may be allowed to repeat that the nature and the quantity of the *fusel-oil alone* make a spirit more or less objectionable from a hygienic point of view, and that there is not the slightest reason for objecting to the use of rectified potato or grain spirits instead of wine-spirit. On the contrary. With the modern appliances, which a professional distiller in Europe is compelled to use at present, for economy's sake, such spirit is produced of a greater purity than any other kind, and it is this spirit chiefly which is used

for making up not only brandies, etc., but also for strengthening wines, not on account of its cheapness only, but for the sake of its purity.

Much has been said against a practice which is universal at home, that of making up brandy by mixing it with other spirits, obtained from grain or potatoes, or by manufacturing it entirely from the latter and flavouring it by the addition of some essence.

But how can it be otherwise, when the demand for the article far surpasses the production? Since the phylloxera has made such ravages with the vines of France, the production of genuine French brandy has fallen from about 60,000 leaguers in 1875 down to 842 leaguers in 1879 and has only risen again to 2,220 in 1882. It is obvious, as Monsieur Dujardin-Beaumetz says, that a millionaire only is able to procure nowadays the genuine article for his table, and, paying even as much as a guinea for a bottle, he is not certain that he gets a really pure article.

The consumption of brandy not having decreased since that time, it stands to reason that the bulk of the article which is in the trade at present, including the favourite brands so largely used in this colony, is made up artificially. What a wonderful chance is offered here to a wine-producing country like the Cape!

But I must state at once that from a hygienic point of view—and I have here nothing to do with the commercial side of the question—this spurious brandy, made from the vile potato-spirit, is by far preferable to the genuine article as the Cape-farmer produces it at present, for the former is free from the more toxic alcohols which the latter generally contains.

It has been suggested that the evil is principally due to the want of age of our brandy. That is true to a certain extent; but never will such brandies as I have met with here, become pleasant and wholesome, even if kept for twenty years. The want of care during the fermentation of the wine and the gross negligence during its distillation impart to it such an amount of fusel-oil and empyreumatic products that no length of time can remove them.

Concluding my little paper, I feel bound to touch another side of the subject before us.

What steps should our legislature take with regard to this adulteration-question? All the greater States of Europe have found it necessary to promulgate laws against the numerous adulterations to which articles of food and drink are subjected. In principle these

various laws are similar to the British Food and Drugs-adulteration Act of 1875. They forbid not only the sale of any food or drink, which contain ingredients injurious to health, but also the sale of any such goods, which are not of the nature, substance and quality indicated by the name. They inflict comparatively high penalties on the man who sells a mixture of milk and water under the name of milk, or coffee mixed with chicory under the plain name of coffee, or oleomargarine, alias butterine, for butter. The German Supreme Court at Leipzig for instance confirmed only lately the sentence of several months' imprisonment, which a Provincial Court had passed on a spirit-dealer. And this man had done nothing but manufacture three leaguers of Jamaica-rum out of one leaguer of the genuine article. It is doubtful whether our legislature is already sufficiently impressed with the necessity of protecting our pockets against such tricks of dishonest tradesmen, but I think that it is the duty of the legislature of every country to protect the lives and the health of its inhabitants.

Some people may say that nobody is compelled to drink brandy, but we must deal with men as they are and not as they ought to be. The many efforts put forth by various governments to check the consumption of distilled liquors have thus far failed. Neither increased taxation of spirits nor severe penalties against drunkenness have availed and in spite of the strenuous efforts of the temperance societies the number of those who stick to alcoholism is immense. But if we cannot abolish the use of alcohol we can very well diminish the dreadful consequences of it by preventing the sale of a stuff which is overcharged with health-destroying ingredients, and I dare to express the hope, that the time is not very far when the sale of food and drink which are detrimental to human life will be as great a crime at the Cape as it is in London, Paris or Berlin.

APPROXIMATE TIDE-CONSTANTS FOR TABLE BAY AND ALGOA BAY.

By W. H. Finlay, M.A.

[Read 31st August, 1887.]

At the suggestion of H.M. Astronomer, D. Gill, Esq., self-registering tide-gauges of the most approved pattern were erected in 1880 at Cape Town and Port Elizabeth by the respective Harbour Boards : and the tracings of several years' tides are now available for reduction. It is to be hoped that arrangements will soon be made for a complete discussion of these curves and for an accurate prediction of future tides. Meanwhile I have thought it would be of considerable interest to obtain some preliminary results with regard to the more important of the tidal constituents, and to see in the case of Table Bay whether any correction is indicated to the data published in the *Shipping Gazette*, 1853, and used for the formation of the tide-tables to the present day. These data were derived from 4,099 observations of the time of high water at the Naval Yard, Simon's Town, during the years 1840-1848. From previous observations in 1834 and 1835 with two temporary gauges, one at Simon's Bay and the other at the South Jetty, Table Bay (near the Imhoff Battery), the conclusion was arrived at that there was no sensible difference between the time of high water in Simon's Bay and in Table Bay : and the results of the Simon's Bay observations were adopted without alteration. No information, however, is given in the *Shipping Gazette* about the heights of the different high waters. Tide-gauge diagrams have, I believe, been regularly filed for many years previous to 1880, but no attempt (so far as I know) has ever been made to discuss them.

The height of the tide at any place may be expressed as the sum of a number of simple harmonic functions of the time, the periods of the functions being the same as the periods of certain terms in the theory of the Sun and Moon. The analytical expression for such a function is

$$A \cos nt + B \sin nt, \text{ or } R \cos (nt - \varepsilon),$$

where t denotes the time elapsed from some starting-point, n is the

'speed' of the tide (so that the function repeats itself in a time $\frac{2\pi}{n}$), R is the amplitude and ϵ the epoch. Each such component is called a tide. The actual height of the tide therefore at any time t is equated to the expression

$$A_o + R_1 \cos(n_1 t - \epsilon_1) + R_2 \cos(n_2 t - \epsilon_2) + \&c.$$

where A_0 is the height of mean sea-level above some assumed datum-line; and the solution of a large number of equations formed in this way determines the most probable values of the constants involved.

Prof. G. H. Darwin has shown that it is possible to deduce very approximate values of six of the most important tides from observations taken every hour for a month. These are the lunar, solar and luni-solar semi-diurnal tides; and the luni-solar, lunar and solar diurnal tides: they will be referred to by the letters M_2, S_2, K_2, K_1, O and P respectively. Of these the two first are usually much the largest and most important, and they are the only ones that can be taken into account in forming a *mean* tide-table to apply approximately throughout the year. But it should be understood that such a table must necessarily be rough: the diurnal tides are neglected and will have the effect of making the predicted times of high water differ from the observed times by about the same amount for the morning and evening tides but in different directions, and there will be inequalities depending on the moon's declination and distance from the earth. If it be thought worth while to allow for these inequalities, the results of the present investigation will allow of its being done: but as a month's observations are not sufficient to determine the elliptic tides and as the time and height of high water are to some extent dependent on the meteorological conditions obtaining at the time, it is perhaps scarcely worth while to incur the extra labour of the computations. For Table Bay I have worked up two series, one in 1885, November, and one in 1887, May: the results from the two series showed a most satisfactory agreement. For Algoa Bay I have taken one series in 1886, April. I have assumed that the height of the tide is affected to the extent of one foot by a change of one inch in the barometer reading, and corrections have been applied to reduce the readings to a mean height of the barometer of 30 inches.

Putting H for the semi-range of a tide, and K for the constant angle of retardation or 'lag,' I find

TABLE I.

	Table Bay.	Algoa Bay.
'Age' of tide	$= 26^{h}\cdot 4$	$= 32^{h}\cdot 0$
Mean height above datum-line	$= 2\cdot 37$ ft	$= 2\cdot 34$ ft
M_2	$H_m = 1\cdot 60$ ft, $K_m = 67°$	$H_m = 1\cdot 76$ ft, $K_m = 97°$
K_1	$H' = 0\cdot 19$ ft, $K' = 124°$	$H' = 0\cdot 16$ ft, $K' = 146°$
S_2	$H_s = 0\cdot 68$ ft, $K_s = 93°$	$H_s = 0\cdot 83$ ft, $K_s = 128°$
O	$H_o = 0\cdot 06$ ft, $K_o = 250°$	$H_o = 0\cdot 05$ ft, $K_o = 280°$
K_2	$H'' = 0\cdot 18$ ft, $K'' = 93°$	$H'' = 0\cdot 23$ ft, $K'' = 128°$
P	$H_p = 0\cdot 06$ ft, $K_p = 124°$	$H_p = 0\cdot 05$ ft, $K_p = 146°$
'Mean' establishment	$= 2^{h}\ 19^{m}$	$= 3^{h}\ 21^{m}$

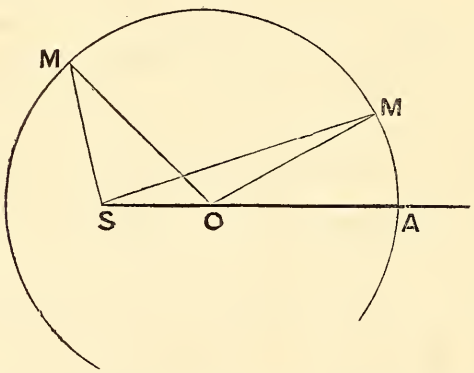
The diurnal tides are therefore small at both places, but they are quite sensible.

The 'mean' establishment hitherto used for Table Bay is $2^{h}\ 43^{m}$, so that the considerable correction of 24^{m} must be applied to the average of the predicted times of high water. This correction to the old value may be due partly to the different position of the gauge in the Bay, but more probably to an error in the statement that the times of high water at the Naval Yard, Simon's Bay, and at the South Jetty, Cape Town, are coincident: for unless special precautions are taken in the erection of a tide-gauge to annul the effects of wave-disturbance it is no easy matter to determine the time of high water from the diagram to a good many minutes.

The 'vulgar' establishment, or time of high water at full and change of the Moon, is $2^{h}\ 33^{m}$. The 'vulgar' establishment given for Algoa Bay on the charts is 4^{h}, but Capt. Skead, R.N., has pointed out that the observations on which this result depends were taken at a station some way up the river and that the value for the Bay should be considerably less: my results make it $3^{h}\ 39^{m}$.

A 'mean' tide-table, sufficient for purposes of approximately predicting the time of high water, can easily be formed by a graphical process from the above results for the M. and S. tides.

The local mean time of the Moon's transit is to be converted into angle at the rate of 30° to the hour and from it the angle ($K_s - \frac{30}{29} K_m$) subtracted : call the result x. Then on any convenient scale take a straight line SO equal to H_s and produce it to any point A. With O as centre and H_m as radius describe a circle : and from the point O draw a straight line OM making with AO an angle equal to x and cutting the circle in M.



Then SM is the height of the tide, and the angle OMS converted to time at the rate of 29° to the hour is the correction to be applied to the mean establishment to find the interval by which the time of high water follows the moon's transit over the meridian.

The correction is subtractive if x be less than 180°, additive if greater.

Table II is such a table ; it gives for Table Bay and Algoa Bay the time of high water and the height of the tide above the datum-line corresponding to each half hour of the Moon's time of passing the meridian. The hours of the Moon's transit are reckoned from 0^h to 24^h, commencing at noon : and it is to be understood that when the time of transit is greater than 12^h the corresponding time of high water is to be increased by 12^h.

Table II.

Local Time of Moon's Transit.					Table Bay.		Algoa Bay.	
					Time of High Water.	Height of Tide.	Time of High Water.	Height of Tide.
h	m		h	m	h m	ft	h m	ft
0	0	or	12	0	2 33	4·61	3 39	4·87
0	30	or	12	30	2 54	4·65	3 59	4·92
1	0	or	13	0	3 15	4·65	4 20	4·93
1	30	or	13	30	3 36	4·61	4 40	4·91
2	0	or	14	0	3 57	4·55	5 0	4·84
2	30	or	14	30	4 19	4·46	5 21	4·74
3	0	or	15	0	4 41	4·34	5 42	4·61
3	30	or	15	30	5 4	4·19	6 4	4·45
4	0	or	16	0	5 29	4·03	6 28	4·26
4	30	or	16	30	5 57	3·86	6 54	4·05
5	0	or	17	0	6 28	3·71	7 23	3·84
5	30	or	17	30	7 5	3·51	7 57	3·63
6	0	or	18	0	7 48	3·38	8 38	3·44
6	30	or	18	30	8 37	3·30	9 28	3·31
7	0	or	19	0	9 30	3·30	10 25	3·07
7	30	or	19	30	10 20	3·37	11 20	3·34
8	0	or	20	0	11 3	3·50	12 8	3·49
8	30	or	20	30	11 40	3·70	12 46	3·68
9	0	or	21	0	12 11	3·84	13 19	3·91
9	30	or	21	30	12 39	4·02	13 47	4·11
10	0	or	22	0	13 4	4·18	14 13	4·31
10	30	or	22	30	13 28	4·33	14 36	4·50
11	0	or	23	0	13 50	4·45	14 58	4·65
11	30	or	23	30	14 12	4·55	15 19	4·78

If we desire to take account of the parallactic and declinational corrections we may proceed as follows :—

For SO (in above fig.) take the value 1·086 $p_{/}$ $\cos^2 \delta_{/}$ H_s where $p_{/}$ is the cube of the ratio of the Sun's parallax at the time under consideration to the mean parallax, and $\delta_{/}$ is the Sun's declination. Then from Tables III and IV take the small corrections depending on the Moon's parallax and declination, remembering that these corrections are to be taken out for the Moon's position at a time anterior to the high water by the 'age.'

TABLE III.

Argument—Minutes of Moon's Parallax in excess or defect of 57′ 2″.

′	$\delta_{/}$ R_m = ± ft
1	0·08
2	0·17
3	0·25
4	0·34
5	0·42

TABLE IV.

Argument—Moon's Declination.

☾'s Dec. N. or S.	δ_2 R_m.	δ_2 K_m.	$\delta_2 t$.
°	ft	°	h
0	+0·12	+2·0	+0·07
5	0·11	1·8	·06
10	0·08	1·3	·05
15	+0·02	+0·4	+ ·01
20	−0·05	−0·8	− ·03

The height and time of high water can be found from the figure (as in the general case) if we substitute

$$H_m + \delta_1 R_m + \delta_2 R_m \text{ for OM}$$
$$x + \delta_2 K_m \text{ for the angle } x$$

and add the time $\delta_2 t$ to the mean establishment. We can also easily find an approximate value of the largest diurnal tide :—

Let t be the time of H.W. and h the height of any afternoon tide computed from the mean tide-table. Find ζ' (= K′ − Sun's long. + 90°) and subtract it from t converted into angle at the rate of 15° to the hour: call the remainder λ: then $\frac{H' \sin \lambda}{h}$ is the correction to t in decimals of an hour.

and $H' \cos \lambda$ is the correction to h.

The corrections to the morning tide may be taken as the same in amount but of opposite sign.

THE BAROMETER: ITS REDUCTION TO SEA LEVEL.

By A. G. Howard.

[Read 28th September, 1887.]

The subject of my paper is the reduction of an indicated atmospheric pressure at an elevated station to that which it would be were the earth below that station removed, and the observation of pressure made at the sea level. The object of thus reducing the indicated pressures to one datum, is for the purpose of charting a number of simultaneous observations for comparison and for the drawing of isobars or lines of equal pressure. The datum employed is usually the mean sea level.

The true formula for this reduction has not yet been developed, and it is my firm opinion, that as far as reductions of pressures on isolated mountains are concerned no formula will ever be produced for giving the true sea-level equivalent. The local eddies, and alterations of temperature, pressure and humidity are such, in such a position, not only at the station itself but at varying heights, that the formula which would be correct at one time would give very erroneous results at another.

But on elevated table lands the conditions are totally different; in such a position there are no local eddies whirling up or down, no atmosphere of varying temperature and humidity below; whatever the reduction is calculated to be, it can only vary with the pressure; the temperature correction has already been applied to the column of air above the station; consequently the conditions are very much simplified.

The formula of La Place takes into consideration the mean temperature of the column of air below the station, but this is not always so easy to find. The actual mean is not always the mean of the two stations. The temperature variable makes a great difference in the reduced reading, and is often very misleading; so much so was this the case, that in the year 1872 the Chief Signal Officer in America ceased to use this variable for high stations and substituted an annual constant. But still at stations of from 2,000 to 5,000 feet in altitude,

the same trouble was experienced by using the temperature variable, and in the year 1881 a system for all stations was introduced, by using monthly constants of reduction to sea level based on monthly normal pressures and temperatures, but this has features quite as objectionable as any other mode.

At the third meeting of the International Meteorological Committee held at Paris in 1885, the late General Hazen of the American Signal Service submitted a scheme for the reduction to sea level, by using for the temperature variable, the mean of the observed temperature and those of the two preceding observations where tri-daily observations are taken, and where only one observation a day is telegraphed to send the mean of the hours 7, 2, 2 and 9. The idea as far as I can see is to use the progressive mean of the day. Mr. Leyst* pointed out the error of this mode of reduction, which instead of obliterating the diurnal curve increased and misplaced it. He still considers that the formula of La Place should be adhered to, and the constants and variables revised according to the light of modern science, and furthermore that the formula should be critically examined with a view to constructing tables to avoid the use of logarithms. He professes to have solved this problem himself. This may be; but I do not believe, even at the best, that it will be reliable for great heights.

Rühlmann, who has corrected the formula of La Place, makes the temperature answerable for all errors of reduction and alters his constants accordingly. But this, I think, is not a very scientific proceeding.

Leyst again points out that humidity has a considerable influence on the pressure. So, I say, has the direction of currents, whether they are rising or falling. The reductions in the centres of cyclones and anti-cyclones would be totally different.

Latitude also makes a difference. According to Leyst; at an altitude of 6,500 feet, a difference of 80° in latitude makes a difference of ·034 of an inch in the pressure. At the same altitude and with a temperature of 20° centigrade, a difference in the relative humidity of eighty-seven, causes a difference in pressure of ·063 of an inch. Hence these factors are to be taken into account, and when we consider that temperature and humidity vary considerably at different heights, and that at a high station such variations are totally

* Pawlowsk Observatory.

unnoticed, I fail to see that the formula of Leyst will be of greater value than that of La Place. As I said before, the conditions are totally different on elevated table lands, and it is principally to these I shall devote my paper.

No formula will ever be reliable for mountain stations higher than 2,000 feet, and for these La Place's as corrected is, and always will be, the best.

In reducing to a datum, it is not absolutely necessary to use the sea level, almost any horizontal plane can be used, and the readings reduced either up or down to it.

But there is a great difference in the results to be obtained by the two modes of reduction. By reducing up we obtain a chart cutting through various conditions of atmosphere, owing to the datum plane being at varying heights above the ground ; at some points it will touch the ground, and at others, be at varying distances above, 500 feet, 600 feet, or 5,000 feet or more.

Consider the varying conditions of the atmosphere at these different heights.

I mentioned in a former paper that the storm-bearing stratum of the atmosphere flows up and over the land where the slope is gradual, but flows round mountain peaks ; so that over the Colony, the height of this stratum would be approximately the same at Cape Town and at Kimberley. Now consider this stratum to be 3,000 feet thick, and let us assume it is very dense, while above flows a current of warm air. A chart plotted on the plane of Kimberley would not represent the actual condition of the atmosphere if reduced up from surface temperatures.

Upon a chart reduced to sea level, the pressure at Kimberley is least, consequently the gradient is from Cape Town and the wind is south-west.

But by reducing up to the plane of Kimberley 4,042 feet above sea level, using the surface temperature, minus the diminution for 2,021 feet, the reduced pressure would be much too low, the gradient being from Kimberley, so that the wind would be shewn to flow to a high area instead of to a low. This will be better understood if we consider the temperature at Cape Town and Kimberley to be 60°. The correction reduced down for Kimberley would be with a variable of 67°, but for Cape Town reduced up it would be 53° or a difference of 14°, causing a difference in pressure of ·090 of an inch. Thus if by reducing down Cape Town reads 30·05 inches and Kimberley 30·00

inches, by reducing up Cape Town will read 25·94 inches, and Kimberley 25·98 inches, thus reversing the gradient.

In reducing down to the sea level the conditions are totally different. It is true that this is an attempt to delineate an impossibility, but of the two impossibilities it is the least objectionable.

If we assume the whole of the solid earth above the mean sea level to be so much atmosphere, perfectly tranquil and unaffected by temperature, currents or any movement, and in layers of diminishing density, the atmosphere itself rolling over it as it does over the solid earth. If we again assume that the weight of the atmosphere above this mass affects it by compression, so that the surface pressure shall bear a proportional ratio to that at the sea level, we have data for constructing reduction tables, for varying heights, provided we know the decreasing ratio of atmospheric density.

It has been demonstrated that the density of the atmosphere decreases with the squares of the distances, and that the number representing this decrease of density obtained by dividing the pressure at any altitude into 30 inches, increases in the proportion of the power of the quotient of altitudes, or in other words, if B represents the upper pressure then log. $\frac{30}{B}$ increases directly as the altitude, so that by applying a correction to the height, the log. of $\frac{30}{B}$ will be obtained.

Hence we have the resolvable equation—

$$H = \log. \frac{30}{B} kg \qquad (1)$$

Where H is the altitude in feet, and B the pressure at that altitude, k is a constant and g the latitude correction.

To find k, let a represent log. $\frac{30}{B}$. Then, omitting g we have, $ak = H$, and consequently—

$$k = \frac{H}{a} \qquad (2)$$

So that by taking the mean of a number of readings at two stations, the one at sea level and the other at a known altitude, we can easily work out k. I have calculated this from readings at Graham's Town and Port Alfred, and find it to be 65,000. This number varies slightly with the sea-level pressure, varying from 64,900 to 65,100, from a pressure of 29 to 30 inches. It is also the same for all altitudes.

The latitude correction has now to be considered. The weight of a body on the earth, decreases from the poles towards the equator in the proportion of $W\left(1 - \frac{\cos^2\phi}{289}\right)$ so that the weight of the atmosphere below any altitude, or the term 30 − B will *increase* in that proportion towards the poles, hence the latitude must be replaced by the co-latitude, and as the cosine of the co-latitude is the same as the sine of the latitude, we have the latitude correction as $1 + \cdot00346 \sin^2\phi$. Colonel Clarke from pendulum observations has altered the co-efficient to ·002606.

The formula for heights based on the foregoing is

$$H = 65{,}000\ (1 - \cdot002606 \sin^2\phi)\ \log.\ \frac{B}{B'} \qquad (3)$$

and for reduction

$$\log.\ \frac{B}{B'} = \frac{H\ (1 + \cdot002606 \sin^2\phi)}{65{,}000}. \qquad (4)$$

The reduction to the gravity of latitude 45° is given by La Place as $1 + \cdot00284 \cos 2\theta$.* Professor Archibald, from Colonel Clarke's observations, has altered this to $1 - \cdot002606 \cos 2\phi$.

Between Cape Town and Kimberley this correction for gravity makes a difference of ·013 of an inch, so that the reduced readings at the latter should have this correction applied subtractively.

The foregoing gives the argument upon which I have considered this question. Of course the formula given will only be applicable for stations on elevated plateaus. For mountain stations they will not answer, as temperature and humidity have such different effects at such stations to what they have on elevated table lands. Moreover the atmosphere between the station and the foot of the mountain has to be taken into consideration with all its varying conditions. For these stations I would introduce the temperature correction $t' = t + \frac{h}{2c}$ where t represents the mean temperature of the date of observation for a great number of years, and $\frac{h}{2c}$ the variation due to altitude, c being the number of feet in which the temperature decreases one degree. This variable c is different for different latitudes and temperatures.

In the tropics the perpetual snow limit is 16,000 feet above the sea level. Taking this as a datum we find that with a sea level temperature of 80°, the decrease is 1° in every 333 feet of altitude, but with a

* Co-Latitude.

sea-level temperature of 60° the decrease is 1° in every 571 feet. The height of the limit of perpetual snow decreases towards the poles in the proportion of the squares of the cosines, and the altitude wherein the temperature decreases 1° will vary accordingly.

By the term tropic I mean that parallel of latitude where the sun shines vertical. The calculation for decrease must be made from this parallel and not from the equator, bearing in mind that solar action lags behind nearly a month. The decrease of temperature due to altitude which I have mentioned is the mean decrease ; the nearer the surface of the earth, the quicker does the change take place. This question of decrease of temperature ought to be gone into practically, and I should like to see it done here. We have Table Mountain and Lion's Head. Why not utilize them ? A wet and dry bulb on Signal Hill would help. Where there's a will there's a way, and I should like to see South Africa take off the palm in this question.

I will now pass on to some practical results. My first attempt to construct charts from barometer readings at high altitudes, was early last year. I compiled thirteen charts for 1885, twelve monthly means and a yearly one. I used Hazen's tables of reduction, and was very successful.

On attempting to plot daily charts, I found that temperature, which had been reduced to a mean in the monthly ones, seriously affected the reductions. One station had too high a reading, another too low, consequent upon the height of the thermometer, and the greater the altitude the greater the error. .

I was so annoyed at this, that I destroyed the whole of them, and plotted new ones, using only those reductions for a temperature of 62°. By these means the differences were done away with, and I came to the conclusion that temperature had nothing to do with a formula for reduction on elevated plains. I then began to work out tables of my own.

The result of my work I now place before you. I do not assert it is perfection, nor do I say the formula is new. A new formula is not wanted. As I said before, mine is to be used solely in reducing from table lands, and by using it we do away with the discrepancies due to the temperature variable, and with the anomaly of a temperature correction where no atmosphere exists.

I have compiled tables of my own for all the principal places in the Colony from which returns are received, and have been successful so far.*

* Charts for the first six days of January and of July, 1885, were exhibited at the meeting.

In conclusion I will give two cases, to show how erroneous it is to take temperature into consideration on elevated table lands. Assume two stations ten miles apart, situated on a plain, both having the same altitude and barometer reading, while the temperature of one is 10° warmer than the other. Upon a chart plotted on the plane of these stations both pressures would be alike, which is the truth, but upon their being reduced to sea level they are totally different, which is an untruth, but by my mode of reduction the truth would exist, even at the sea level.

The other case is this. Often when pressure has been very steady all over the Colony, the diurnal range of temperature at an elevated station has been very great. Now if the readings had to be reduced to a steady sea level pressure the upper barometer must have oscillated very much, but as such was not the case, the effect of temperature must have been *nil*. Major Pinto, a traveller who crossed Africa from Benguela to Natal, notices this. He says : "Whoever bestows any attention upon the meteorological observations I publish, will see that the atmospherical changes in this part of Africa have but a slight, if any, influence upon the pressure, which remains the same amid the most sudden variations."

And again he says : "An investigation of these tables will shew the great uniformity of the barometrical oscillations and the enormous inequalities of temperature and humidity of the air in the countries to which they refer." The country Major Pinto refers to is the great table land of Central Africa, far from all influences of atmosphere *below* the places of observation.

ADOLPH G. HOWARD

Cape Town, 28th September, 1887.

THE ACACIAS OF SOUTHERN AFRICA.

By R. Marloth, Ph.D., M.A.

[Read 26th October, 1887.]

There is scarcely any book on travel in Southern Africa that does not contain some pages dedicated to the description of thorns and spines on plants. Although most of our travellers took very little interest in botany, they could hardly help paying considerable attention to a subject that was forcibly obtruded upon them every day. In the coast districts of the Cape Colony and in the regions extending towards the north-east spiny bushes or trees take a less prominent place in the vegetation of the country, and those armed with such weapons belong to several very different families. In the central regions however and especially in those vast tracts which extend north of the Orange River far beyond the tropic of Capricorn, one family, or to speak more properly one genus, of thorny trees comes to the front and impresses its character on the landscape. This is the genus *Acacia*. Two species of acacias are familiar to all of us, namely the common Karroo-thorn and the so-called Port Jackson willow. The latter is not indigenous to this country, but comes from Australia, and I may here mention, that the large genus *acacia*, which contains over 400 species, is distributed over three continents: Australia, Africa and America. On our continent it is principally found in the northern and southern countries; but our number of species is much inferior to that of Australia. The Port Jackson willow being a typical representative of the bulk of the Australian acacias, and the Karroo-thorn one of ours, the difference between the two branches of the genus is apparent.

There are nineteen species known from South Africa, six of which belong to the eastern coast districts, namely: *A. arabica*, *natalitia*, *raffra*, *spinoza*, *Kraussiana*, *pennata*, whilst three have been found only in the eastern Transvaal: *A. ferax*, *Burkei* and *eriadenia*. One species only, the above-mentioned Karroo-thorn, reaches our neighbourhood, the remaining nine species being restricted to the Kalahari region, where I collected them all but one. To these must be added seven new species which I discovered in Damaraland, namely: *A. hereroensis*, Engler; *A. trispinosa*, Marloth et Engler; *A. dulcis*,

Marloth et Engler ; *A. Luderitzii*, Engler ; *A. Marlothii*, Engler ; *A. uncinata*, Engler ; *A.* ——— Engler and one new species from Griqualand West. These twenty-seven species are easily arranged in two natural groups according to their inflorescence, the one section bearing the flowers in glameroles, the other one in racemes.

Less scientific, although perhaps sufficient from a practical point of view, is the principle of classification which Baines* mentions as established by Dr. Kirk. He divides them into three groups, viz., species which tear the clothes, those which tear the flesh and those which tear clothes and flesh.

For our purpose, however, I thought it best to adopt another plan and to deal firstly with the shrubby species and then with the trees, putting aside the two climbers (*A. pennata* and *Kraussiana*).

There are altogether fifteen species of the shrubby form, out of which number I may mention the seven most remarkable types.

A. stolanifera, Burch., the lowest of all of them, is common in Griqualand West. The branched underground stem sends up numerous straight twigs, 3 to 4 feet high, which, growing close together, form pretty little islands of fresh verdure on the rufous soil.

A much greater influence on the features of the country is exerted by *A. detineus* Burch., the *Haakedorn* of the colonists. It is a bush 6 to 10 feet high, sometimes forming thickets to the exclusion of all other shrubs. Such places are very trying to the traveller and particularly so to his oxen and the canvas-cover of his wagon. I cannot better illustrate this obtrusive plant than by quoting Burchell,† who named it *detineus* out of mere revenge. He made his first acquaintance with it shortly after he had passed the Orange River and he describes this interview as follows :

"I kept with two of the Hottentot waggons, but they led me through such close thickets of thorny bushes, and which seemed ready every minute to tear away the canvas from the tilts, and our clothes from off our backs, that I had given up all hopes of finding our way out of them till the morning. . . . The largest shrubs were about 5 feet high. . . ., and it is the same thorny bush which gave us so much annoyance the night before where it was above 7 feet high. I was preparing to cut some specimens of it ; which the Hottentots observing, warned me to be very careful in doing so, otherwise I should certainly be caught fast in its branches. In consequence of

* *Th. Baines.* Explorations in South Africa, p. 147.

† W. J. Burchell, Travels in the Interior of Southern Africa Vol. I., pp. 308 and 309. London, 1822.

this advice, I proceeded with the utmost caution, but, with all my care, a small twig caught hold of one sleeve. While thinking to disengage it quietly with the other hand, both arms were seized by these rapacious thorns, and the more I tried to extricate myself, the more entangled I became ; till at last it seized hold of the hat also ; and convinced me that there was no possibility for me to free myself, but by main force and at the expense of tearing all my clothes. I therefore called out for help, and two of my men came and released me by cutting off the branches by which I was held. In revenge for this ill-treatment, I determined to give to the tree a name which should serve to caution future travellers against allowing themselves to venture within its clutches."

I found a larger variety of this species in Damaraland which forms there bushes often some 20 feet high.

A. trispinosa Marloth et Engler, a new species from Damaraland, is a low bush, 3 to 6 feet high. I named it *trispinosa*, on account of it having three spines under each leaf. As the specimens show, this species possesses not only the two recurved hooks of the hookthorn, but also a third one between these two, which is bent in the opposite direction. It is a pity that Burchell did not make the acquaintance of this bush, for he would have certainly supplied us with a graphic account of the superiority of such an arrangement over the simple system of hooks on the hookthorn.

Another new species from Damaraland is *A. dulcis Marloth et Engler*. It forms bushes of nearly the same dimensions as the Damaraland variety of the hookthorn, but it is easily recognized by its white or reddish-white bark. It is remarkable for its producing a gum, which is quite different from the common acacia-gum, being sweet and eatable, so that it serves as a regular sweet-meat to the natives. Hence the name *dulcis*.

A. haematosylon Willd. forms shrubs or little trees, 5 to 10 feet high. Burchell found it near Griquatown, since which time it has not been recorded. I saw it in the neighbourhood of Kuruman. It differs from all the other species by its slender and elastic branches, drooping like a weeping willow.

A. Oaffra Willd. a native shrub of the Eastern districts may be mentioned, not on account of its weapons, which are comparatively small, but for its much larger leaves.

As the last member of this series I have to mention another new species from Damaraland, which I had named *inermis*, on account of its having no spines or prickles whatever, but the name of which has

been changed by Prof. Engler to that of *A. Marlothii.* Amongst all the South African species this is the only one which is unarmed.

There are eight species of acacias growing to the size of considerable trees.

I mentioned already the common Karroo-thorn (*A. horrida Willd.*), which abounds on the banks of rivers and water-courses in the countries east and north of us. It forms beautiful groves ; for instance, on the banks of the Orange River, as may be seen from the charming painting in Burchell's book above quoted. It does not, however, always grow in such clumps of slender stems. In the Eastern Province, for instance near King William's Town, numerous isolated trees stand on the slopes of the hills, reaching a height of 20 to 30 feet, and in Damaraland I saw trees 50 feet high. Its name *horrida* has not been given without sufficient reason. The geminose spines, as white as ivory and often as long as a finger, are as sharp and pointed as a needle. Hence the branches of this acacia are very welcome to the traveller when he is obliged to erect a kraal for his cattle, for even the roaring of a lion will not terrify his oxen to such an extent that they would break through a fence of this kind.

Another species, *A. hebeclada Dc.*, is also of a very variable size. Generally occurring only as a shrub, a few feet or yards high, it forms on more favourable places considerable trees with a trunk as thick as a man's body. This species, I think, boasts of more formidable weapons than any other one, for its strong and sharp hooks are often an inch long, and, being covered with a fine tomentum, not only lacerate the flesh, but produce a very painful itching, which lasts long after the wound has been inflicted.

Straight spines and recurved hooks are combined on another acacia, which Burchell named appropriately *A. heteracantha*, although there are several other species armed in the same manner. There is, however, no specimen known which with safety can be referred to this name. I thought first that a species pretty common in Griqualand West should be considered to be Burchell's *A. heteracantha*, but the shape of its legumes differs so widely from B.'s description, that it has been necessary to give it another name (*A. spirocarpoides Engler*). It is only 15 to 20 feet high, but another species, similar in its external appearance and differing also by its legumes (*A. Maras Engler*), grows to a lofty tree of 40 to 50 feet, adorning the valleys of Damaraland. These three species are, as I suppose, the *Hauk en steek* of the Dutch colonist, for the long spines prick the unfortunate victim which the hooks prevent from escaping.

The most important South African representative of this genus is the camelthorn, which name, however, is applied to two nearly allied species, the *A. Giraffae* of Burchell and the *A. eriolaba* of Meyer. The first one, the *Mokáala* of the Betchuanas, was formerly common enough in the Free State, Transvaal and Bechuanaland, the other one, the *Omumbonde* of the Hereros, takes the place of the former in the western parts of the Kalahari region. I said, that the *Mokáala* was formerly common in the countries north of the Great River, but it is not so any more. For more than fifteen years all the steam-boilers of the Diamond Fields have been heated with its wood, which almost equals coal in respect to its heating power, and it is therefore not surprising that this tree has nearly disappeared from the country within several hundred miles round Kimberley. Before the completion of the railway to Kimberley, thirty to forty wagon-loads of this wood were brought to market every day, which amonnts to a yearly consumption of 5,000 tons. The wholesale destruction of the *Mokáala* in a country which is almost deficient in other trees of remarkable size is very much to be regretted, for it has considerably increased the dreariness and monotony of these regions. A full grown *Mokáala* is a beautiful tree. It is not often over thirty-five feet in height, but the thick trunk, two or three feet in diameter, bears an umbrella-shaped crown, resembling thus in the distance the famous stone-pine of Italian and Greek landscapes. In former times hills and plains of those vast northern territories were adorned with scattered *Mokáalas* and the sun-burnt traveller could pull up his wagon under its shady roof, but now, having once left the willow-fringed banks of the Orange and Vaal rivers, he may pass along for many a day without perceiving a single tree rising above the shrubs of the hookthorn and vaalbosch (*Tarchonanthus minor Burch.*).

The other camelthorn, the *Omumbonde*, reaches the same dimensions as the *Mokáala*, but it never assumes such picturesque form having more resemblance to an old knobby oak.

Both trees differ from all other acacias in the shape and size of their pods.

The last acacia which I intend to deal with here is the *Ana-tree* (*A. albida Del.*). It is not a South African species properly speaking, occurring principally in Western Central Africa and in the regions of the Upper Nile, but it comes within our limits on the northern border of Great Namaqualand. It is the biggest of all our acacias, having a trunk often four feet in diameter and a height of sixty feet. The large crown is generally hemispherical with fresh, rich foliage,

and such trees, which abound along the river beds of those countries, afford therefore much better shelter against the sun than any other species.

The legumes of the ana-tree are broad, twisted, and not leathery as are those of the other acacias. They are farinaceous and constitute a very valuable food to herbiverous animals. This is probably one of the reasons for the comparatively wide distribution of the ana-tree, for the hard seeds pass undigested through the animals which have fed on its pods.

Having become acquainted with the principal representatives of this genus, it will be of some interest for us to throw a retrospective glance on the various forms we have met with. We have seen some species which are only a couple of feet high and others which raise their lofty heads high above any other indigenous tree. We have admired the rich foliage of the ana-tree and looked almost with pity at the few poor leaves of the hookthorn. We found a greater variety still in the formation of the pods and spines. The pods are of all shapes and sizes; linear, moniliform, oval or half-moon shaped; straight, bent, sickle-shaped or twisted; thin and papery, leathery, woody or mealy, and from one inch to six inches long.

Fir spines, or in some cases the prickles, are straight or recurved, or both kinds are combined on the same branch; they are short or long, thin or thick, glabrous or pubescent, rigid or elastic. They occur mostly in pairs, one species however having three under each leaf and one species only being entirely destitute of them.

The question arises: What are the causes of such variety of forms in a comparatively small number of species? Or since our knowledge of the matter at present is not sufficient to enable us to grapple with the whole of such a complicated problem, we may at least ask: Can any relation be shown to exist between certain peculiarities of these plants and their surroundings, especially as far as climate, rainfall, soil, etc., are concerned?

We are accustomed to find the larger forms of nearly allied plants in localities which have an ampler supply of water, but this common rule does not hold good in this case, as a comparison between the Karroo-thorn for instance and the camelthorn shows. The former although growing on the banks of perennially flowing rivers, is much inferior in its dimensions to the latter, which prefers dry plains and stony hills.

"It is surprising," says Grisebach in his book *Die Vegetation der Erde*, "that such a delicate thing as the acacia-leaf is able to exist

under the scorching sun of the desert, where all the other bushes bear thick and leathery leaves."

There can be no doubt that the acacia-leaf does possess some means of protection against the detrimental effects of heat and sun, and we find on examining the different species with regard to this point, that the leaves are the very organs where adaptation to circumstances has reached a high degree of perfection. We have already seen how economical for instance the common hookthorn is in the production of leaves as compared with other species of moister situations, but it is not so much the quantity of the foliage as the structure of the leaves, which enable them to exist under such trying conditions.

Almost all the acacias, and especially those which inhabit the desert-regions, contain in their sap a certain amount of gum and the juice of their leaves is more viscid and less watery than that of other plants. It is obvious that this peculiarity of the leaf tends to lessen the loss of moisture, for a slimy, gummy, fluid evaporates more slowly than an aqueous one.

Another very effective arrangement for the protection of the leaf is its periodical motion. In the morning the leaflets open and stand out almost horizontally, but towards 10 or 11 o'clock, when the insolating heat of the sun becomes more intense, the leaflets rise towards their rachis until the two upper sides of each pair touch each other. The twofold advantage gained by this change of position is apparent. The leaflets, turning their edges towards the sun instead of the broad surfaces, are thus enabled to avoid the direct effect of the sun's rays and as each pair then exposes only two sides to the hot air instead of four, their transpiration must be less than in the former position. In the afternoon, when the greatest heat has passed, the leaves open a second time, closing again about sunset on account of the cooler night. In *A. destineus* this arrangement is so perfect, that even large bushes throw almost no shade in the middle of the day and there is nearly as much sunshine under such a bush as at the side of it.

The most striking difference, however, between the species which grow in dry localities and those which are only found where subterranean water is present, is to be observed in the anatomical structure of their leaves. Taking again for comparison the karroo-thorn and the camelthorn and examining transverse sections of their leaflets under the microscope we find that the tissue of the former is formed of large cells with considerable intercellular spaces, and that the

epidermis consists of comparatively large cells with thin outer-walls, but in the latter the cells of the tissue close tightly together and the cells of the epidermis are provided with a very thick outer-wall. This wall measures in the former case only 3 to 5 μ, but in the latter 15 to 18 μ. The Karroo-thorn like the ana-tree grows only along the rivers, or at least on such spots where underground water exists, and they can therefore well afford to lose a considerable amount of moisture by transpiration, but the inhabitants of dry hills or plains, such as the camelthorn and the hook-thorn for instance, must economize the precious liquid and prevent its loss by all available means.

But it is not only against the climate that the acacia-leaf must be protected.

What purpose, the inquisitive observer of nature will ask, is served by all these nasty spines and prickles, which appear to be there only to worry the traveller? The answer to that question is not far to seek. Linnæus already called the thorns the weapons of the plants, and it strikes us at once, that the spines and hooks of the acacias protect their leaves against the attacks of grazing animals. In a desert like the Kalahari where no blade of grass can exist for many months of every year, whilst the country often teems with large game, the delicate leaves of the acacias would be completely destroyed year after year were they not so well protected.

I have made an interesting observation bearing on this question. Young bushes of the karroo-thorn as well as of the camelthorn possess always the longest and most numerous spines, whilst those of older and higher bushes are reduced in size and number. Young twigs of the camelthorn for instance bear spines about two inches long, but on older trees they seldom reach half an inch. As long as the bush is low its leaves must be well protected, but once grown to sufficient height it is no longer exposed to the attacks of game, and the material which is not required for the construction of spines can be employed for other purposes, as for instance, for the production of fruit.

These relations between the acacias of South Africa and their surroundings remind us that if we desire to understand the peculiarities of the vegetation of a country we must study, not only the floral characters of the plants, but also their biology, for the members of the vegetable kingdom are as well adapted to the peculiar requirements of the position which they occupy as animals to theirs. I think it is apparent even from the few observations which I have mentioned above, that this group of plants serves remarkably well to illustrate this natural law.

ON THE SUBJECTIVE CAUSES OF EVOLUTION AS ILLUSTRATED BY THE GEOGRAPHICAL DISTRIBUTION OF PLANTS.

By Professor Guthrie, LL.B.

[Read 25th April, 1888.]

INTRODUCTION.

When a seed germinates the character of the plant that will be evolved depends both on the nature of the seed and on its environment.

The first may be called the *subjective*, the latter the *objective* cause of the plant's ultimate form.

Soil, moisture, light, temperature, as well as man's direct interference in training and pruning, may make all the difference between a stunted pot plant of a few inches high and a magnificent forest tree. But whatever differences may thus be objectively produced there are certain unchangeable subjective characteristics due to the nature of the seed itself which cannot be altered. No cultivation for example will evolve a fir tree from an acorn.

It is now generally believed, and will here be assumed, that existing species of plants and animals have been evolved from dissimilar pre-existing species, and the question arises to what extent the forms of existing species are due to subjective as well as objective causes, that is to say to what extent the forms of existing species are due to the subjective tendency or capability of the ancestral species from which they have been evolved of varying in what may be called *prescribed* ways, or whether there is no such prescribed capability or tendency and whether evolution is due to environment only.

Darwin's theory of evolution by natural selection seems to have left this question open. His own views seem to have undergone a change between the dates of the publication of the first and last edition of the "Origin of Species." At the date of his first edition (1859) he seems to have been disposed to account for evolution almost entirely on objective grounds, whereas at the date of his last edition (1886) he clearly laid more stress on subjective causes of evolution.

It is true that the only subjective element of evolution he expressly

recognises is that of hereditary influence, but he does not seem to deny the possibility of the existence of other subjective tendencies.

My object in the present paper is to enquire how far the geographical distribution of plants and other organisms on the earth's surface throws light on this question. With respect to the distribution of plants the phenomena to be accounted for are as follows :

(1.) Large numbers of identical and of closely allied genera and species of plants often extend over continuous areas possessing more or less uniform physical conditions.

These areas are called Floral Areas. Schouw divides the surface of the earth into twenty-five Floral areas, defining a Floral area as being a region in which :

I. At least half the known species are endemic.

II. A fourth part of the genera are endemic or nearly so.

III. The orders are either peculiar to the region or have their maximum there.

(2.) Identical and closely allied genera and species occur in widely separated areas having similar physical conditions. Thus :

I. Two-thirds of the Antarctic genera and at least thirty species are common to that region and Europe.

II. Between forty and fifty flowering species of Tierra del Fuego are European.

III. Half the genera of extra-tropical South America are European.

IV. Half the genera of New Zealand are common to Europe.

V. Several so-called European species are found on the Australian Alps.

(3.) Regions very similar in climate and other physical conditions have very different Floras : Thus :

I. North America and Europe.

II. Mexico, South Africa and Australia.

III. Tropical Africa, Tropical America and the Moluccas.

There are two theories to account for these facts :

(*a.*) The theory of local origin and migration.

(*b.*) The theory of simultaneous evolutionary development.

These two theories are not absolutely exclusive of each other, but they depend on somewhat different views as to the origin of species.

There are however certain common grounds of agreement which it will be expedient first to mention.

These are as follows .

(1.) There is no essential difference, but one of degree only, between variety and species and between species and genus.

(2.) Existing specific forms ha ve in some way been evolved from specifically different pre-existing forms.

(3.) The process by which existing species have been evolved is wholly or partially what Darwin has called "natural selection" (including in this term for convenience sexual selection also).

The points in which the two theories differ are as follows :

(1.) According to the migration theory variations are due to environment only or to causes so obscure that they may conveniently be ascribed to chance. They are all-round in character and almost infinite in number, and there is no natural tendency or capability of varying in any prescribed directions.

Similar variations *may* be produced simultaneously in different regions from similar environment, but as the combinations of external circumstances are practically infinite, as are likewise the possible variations of the species, the chances of the same varieties and species beingevolved simultaneously and independently are infinitesimal.

The theory of simultaneous evolution on the other hand depends on the following hypothesis :

(2.) Variations, being correlative, are only possible in a comparatively limited number of directions and naturally tend to follow certain lines, so that amidst circumstances not too dissimilar similar variations will occur independently and simultaneously both in the different parts of the same area and even in the different areas over which a species is diffused ; so that a varying species without any necessary intercommunication evolves throughout its area either the same or closely allied species.

According to the migration theory species originating locally have become more or less widely disseminated by migration. The *seat* of the species being generally assumed to be the region in which the species and its congeners are most numerous. Going back to this region and applying the same theory, we have to conclude that the species now distributed over it originated in some limited locality, and continuing the process the species is ultimately traced back either to a number of individuals of the species somewhere simultaneously and independently evolved, or to a single individual ancestor or pair of ancestors, from which all the individuals of the species have come by lineal descent.

The former alternative however seems to be fatal to the migration theory. If in any locality a number of individuals of a species A simultaneously and independently vary either *per saltum* or by degrees, and in the course of many generations into the B form, there seems to be no valid reason why this process should be confined to one locality of the seat or seats of the A species.

If independent simultaneous and similar variation be admitted at all, it seems impossible to deny that it may take place in more than one locality either in the same or different floral areas. The denial of this possibility seems necessarily to involve the hypothesis of species being derived from a single ancestor or ancestral pair.

On the migration theory the existence of the same species in remote areas is accounted for either on the hypothesis of the actual conveyance of seeds or plants over the intervening regions by birds, ocean currents, or otherwise, or on the hypothesis favoured by Darwin and Hooker that the two regions where the common species exist were once united with each other or at different times with some third region by similar climatic and other conditions and thus formed a single floral area, which has since become severed by change in the climate and physical conditions of intermediate regions.

The existence of distinct floral areas under similar physical conditions is not absolutely inconsistent with this theory of the origin of species but presents many difficulties.

European species can and do thrive in America ; and *vice versa*, if their migration on a large scale has been possible between Europe and the Antarctic regions, it is difficult to understand why there should not be a greater community of specific forms in regions so close as Europe, and Northern Asia (which belongs to the European area) and America.

So also if migration were so efficient a factor in the distribution of species as this theory supposes it is hard to understand why so marked a difference should still continue to exist in the fauna and flora of islands belonging to the Moluccan and Australian areas, which are only separated from each other by a narrow strait (see Wallace).

A continuity of physical condition, such as would have connected the Australian Alps and Siberia into one area, would surely have also connected the tropical and sub-tropical regions of Australia and the Moluccas and thus have left far more distinct traces of its existence.

The theory of simultaneous variation accounts for the appearance of the same species both over a continuous floral area and in widely separated areas as follows :

From an ancestral species probably equally widely spread, having everywhere a capability of varying in a limited number of directions and a tendency to vary in still fewer, similar circumstances have over some parts of the area favoured the production and survival here of one variety and there of another. The localities in which the same varieties have been favoured, are not necessarily conterminous or even in the same area. In other places again, circumstances not being favourable to the new varieties, the species has disappeared.

The difference of the flora of regions similarly situated as far as physical conditions are concerned would then be explained as follows :

Two areas, say Mexico and South Africa, have formerly been subject to different physical conditions and have thus developed different specific and generic forms. Owing to change of circumstances the physical conditions of these two regions have subsequently become assimilated to each other. The conditions requisite for the same flora are not then here present. Similar conditions operate on dissimilar forms. In this way not identical but parallel species are produced, resembling each other outwardly but not in the structure of the part on which classification depends. Thus in Mexico we find the succulent Cactaceæ, in South Africa the succulent Euphorbeaceæ.

The theory of the individual or dual origin of species on which the migration theory rests, or rather, perhaps, on which the denial of the plurality of seats for the same species depends, seems to be surrounded with difficulties.

According to this theory either the new species is produced *per saltum* or it is produced from a single line of ancestry which has left no collateral issue.

Either hypothesis is inconsistent with the admitted prevalence of cross-fertilization.

If variations are all-round variations, following no perceptible law and selected by environment only, then where cross-fertilization is the rule every attempt at variation would be swamped, or only those new species would be produced which are self-fertilized, so that cross-fertilization would either become unknown or very rare.

The production of new species *per saltum* is hardly worth serious consideration. In the case of all animals and dioecious plants it would involve the accidental and simultaneous production of two

specifically identical individuals of opposite sexes and specifically differentiated from their ancestors, thus bringing us back practically to the abandoned doctrine of specific creations.

The individual or dual origin of species fails also in satisfactorily explaining the extinction of a varying species.

It seems to be generally held that existing species are collaterally, not lineally, related. Why this opinion is so generally entertained is not very obvious. One reason perhaps is that species are not known to sport into other species, which owing to atavism would sometimes be the case if existing species were related in the direct ancestral line. Now in the absence of a general tendency to variation on one or some few directions, this extinction of the ancestral form is not easily accounted for. If a species diffused over a floral area gives rise to a variety in one particular spot, in the absence of any tendency towards the same variation elsewhere, it seems almost impossible that the new form should supersede the old. As Darwin has pointed out, in order that a species may successfully invade a new region it must not have any closely allied race to contend with.

Successful invaders of an occupied area to a great extent belong to genera not represented in the area they invade, and seldom or never do they belong to species closely allied to species existing there. Still less is it likely that a variety formed in one area will pursue and exterminate the parent species in an area now detached from that in which it originated.

Whatever theory is entertained as to the origin of new varieties must be also capable of explaining the disappearance of the form from which it originated and in this respect the purely objective theory of the production of new forms seems to fail.

According to the theory of variation in a comparatively few directions due to an innate law or tendency, cross-fertilization is a distinct aid to the production of new species and to the extinguishment of the old.

Thus suppose a species A has a natural tendency to vary in a direction which would ultimately produce a new species Z, and let the first step of variation produce the form B. Let A be an annual and let 20 per cent of its progeny take the B form.

Some few of these B's may be cross-fertilized with each other, and their inheriting the tendency to vary from both parents will leave progeny of which the majority will pass into the C form, another step towards the specific form Z. The great majority of the B's, however,

will be cross-fertilized from A's, and about an equal number of the A's from the B's. Of these inheriting on one side the B form and on the other the tendency to the same form, the great majority will be of the B form, while of the A's crossed with A's, another 20 per cent will pass over to the B form. In this way it is obvious that the A form will soon begin rapidly to give way to the B, and subsequent forms tending nearer and nearer to X. We have here been supposing that natural selection does not operate, and that external circumstances are equally favourable to A B and the subsequent forms up to X. If, however, this be not so, if natural selection favours the development towards X, then the evolution of the X species and elimination of the A species will be facilitated. If, however, natural selection be opposed to the B variation, then the A form will simply tend to die out in its attempt to vary in a way in which it is not permitted to vary.

As this tendency to vary is supposed to characterize the species wherever it exists, it is easy in this way to account for the comparatively rapid extinction of a species which is actively varying.

But it may be asked what proof have we of the existence of this capacity to vary in comparatively few directions and tendency to vary in fewer still.

We cannot say that we have any distinct proof of the existence of such laws except in so far as these laws better explain the evolution and extinction of species. Some arguments however may be urged in their support :

(1.) The doctrine of the correlation of organic variation.

This doctrine is not to be confounded with that of the correlation of organs.

Whatever be our theory as to the origin of species or the cause of variation, it is plain that no plant or animal could exist unless its organs were correlated not only with each other but with the environment. Their eyes, teeth, limbs and digestive organs are correlated with each other and the food supply. In order that varieties may subsist they must involve the correlative variation of the necessary organs. The harmony of life must be maintained. Variations opposed to this harmony will be disadvantageous to the individual.

The bare theory of casual variation with natural selection meets this requirement by supposing that among the infinite varieties which are always occurring those only will survive which involve the requisite harmony with each other and the environment.

"Only give us sufficient time," the casual evolutionist would say, "and without requiring any law by which organic variations are harmoniously correlated we can account for the fact that in the outcome they are and must be so related." This is true, but the demand for time becomes excessive.

The correlation of variation of which we are now treating is however something more than this. It is the law by which without reference to natural selection, or the advantage of the individual, one part of an organism cannot vary without involving the variation of other parts also.

This subject has been so fully treated by Darwin that it is hardly necessary to do more than to refer to his works on this subject.

Darwin has shewn that as both animal and vegetable forms, even those most highly developed, consist to great extent of variously developed homologous parts their parts must tend to vary correlatively to each other. Thus, for example, the jaws are related to one of the three modified vertebrae, constituting the skull, in the same way as the fore limbs are to another of these vertebrae and the hind-limbs to one of the sacral vertebrae. Now Darwin points out how breeders have noticed that these parts vary together, so that elongated jaws are generally found with elongated limbs, both fore and hind. Not only however do homologous parts vary together, but the same is the case with respect to parts between which there is no apparent homology, as in the oft-quoted case of white blue-eyed cats being nearly always deaf, and tortoiseshell cats of the female sex.

Again flowers and fruits are modified leaves. The varieties of cultivated fruits have been produced by man's selection with reference to the fruit itself. Yet an experienced horticulturist can distinguish the different varieties from the correlated and unintentional variations of the foliage.

How far-reaching is this correlation of parts is well illustrated by the fact that the experienced *hologist* can reconstruct the entire animal from the fragment of a bone, can tell you not only that it is a bird and not a mammal or reptile, but can tell you to what tribe of birds it belongs, what was its size and its habits. So from a few grains of tenui carbonized wood the phylologist will reconstruct a tree with foliage, flower and fruit.

(2.) The existence of a tendency to vary in certain directions with a capacity of varying in others is well illustrated from the experience of breeders of plants and animals.

Man, selecting for his own purposes, has probably not succeeded in producing a single new species of plant or animal, and probably not a single naturally permanent variety which might be regarded as a step towards a new species.

Unless kept under man's incessant supervision our cultivated plants and domestic animals revert to their ancestral form. Pigeons allowed to breed freely among each other revert to the rock pigeon form, dogs mongrelize towards the " dingo " type.

Not only is this the case, but in proportion as the varieties produced by man's selection deviate more and more from the parent type, so the breed as a rule becomes increasingly difficult to keep up, on account of increased delicacy of constitution and often also diminished fertility.

The explanation of this is that man tries to produce variations which are not in the direction in which the species tends to vary, and the further he proceeds with these variations, which we may call un-natural, the more difficult his task becomes. Not that variations occur less often, for the reverse is the case, but that the variant forms have not those qualities which ensure permanency in the ordinary conditions of life.

Another experience of horticulturists also bears on the same point.

It is well known what difficulty there is to get some wild plants to vary when first taken into cultivation. Some have resisted for generations every attempt to produce desired varieties. At last some lucky or more observant gardener detects in some individual a slight tendency to vary in the desired way. Carefully separating this individual and breeding from it, he finds among some of its descendants a further variation in the required direction ; after this the task is comparatively easy. The descendants of those plants which for two or three generations have varied in one direction vary freely, at first in the same direction, and afterwards owing probably to correlation of organs in other ways also. Thus illustrating the fact that the tendency to *variation is hereditary and produces like effects in many individuals.*

(3.) Another observation tending to the same conclusion is the frequent occurrence of well-marked varieties among wild plants growing side by side with apparently identical environment, and the fact that the same varieties also occur independently and in places remote from each other with different environments.

The collection and classification of marked varieties, especially extreme varieties, sometimes called monstrosities, is at present occupy-

ing many botanists, and the mere fact that such varieties can be and are classified and named, seems to prove that varieties are not all round varieties due to chance (unknown law) but to definite laws, which produce like results in places remote from each other.

(4.) The theory of a law of variation in predetermined directions also renders more intelligible the development of organs so complicated and specialized as the eye.

Darwin admits that when he reflected at what an early geological period eyes of apparently perfect organization were produced, he hesitated long to come to the conclusion that mere casual variation and natural selection could have produced them.

The hypothesis of natural tendency to vary in prescribed directions would remove much of this difficulty.

If we imagine that the early and lower forms of animal life were sensitive to light over their entire surface, we can easily imagine that the localization of this sensitiveness increased in degree, and its suppression elsewhere might be an advantage to the animal. Such a change being the first step toward the development of an eye if occurring according to a law, a tendency to vary in a prescribed direction might within a period, not inordinately prolonged, result in the complete perfection of the organ.

(5.) The migration theory supposes that man originated from a single pair of ancestors belonging to some anthropomorphic species now extinct, and that from this pair of ancestors, the Bushman and the Caucasian have both descended. The theory of simultaneous variation merely supposes that the Bushman and the Caucasian have descended from the same ancestral anthropomorphic species but that neither is the Bushman a degenerated Caucasian nor a Caucasian an improved Bushman, nor that the Bushman and Caucasian descended from common ancestors.

It may be urged against the theory of variation by common innate tendency that it only accounts for the existence of widely spread species by supposing the previous existence of an equally widespread parent species.

This is true, but it is hardly a valid objection.

Geology in no very distinct terms perhaps, but still conclusively enough, seems to tell us that the fauna and flora of the world were formerly less diversified than at present: that species and genera if not fewer in number were at least more cosmopolitan in habit than at present: that for example the fauna and flora of

Europe and America were more closely allied than is now the case, that the same was true of North and South America, and of Africa and India.

If this be granted we have no choice as to whether we are to trace existing species to former equally widely diffused species. The problem is thus set as by nature itself. In passing from previous geological times down to the present the phenomenon to be explained is not so much why it is that some modern species are so widely diffused, but why it is that modern species generally have become relatively more localized, why for example the marsupials once abounding elsewhere should now be almost confined to the Australian Area. Whether we adopt the theory of local origin and subsequent migration or that of simultaneous variation the main outlines of the problem are in this respect the same.

It must be remembered that to advocate the doctrine of simultaneous parallel variation as a cause of the co-existence of the same species in remote areas is not to deny that migration or accidental or intentional transport is a cause also. Where the agency of man comes in, the spread of species by migration is a well-known fact, and it is equally well known that the spores of many plants and the seeds of not a few can be carried great distances by the wind, that some seeds are transported thousands of miles by ocean currents, without losing their vegetative powers, and that birds and even insects must sometimes transport seeds.

On the other hand too much stress must not be laid on the effect of the infinite variety of surrounding conditions or the differentiation of nascent species. This variety is, it is true, in one sense infinite but in another sense it is very limited. Climate, soil, and rivalry in the struggle for existence may be varied infinitely but only in infinitely small respects while in those general aspects which favour or oppose the success of a species, the variations are by no means so numerous, otherwise the same species would not sometimes flourish over such large areas as they do, and in such vastly different conditions as to soil, climate and competition, and would not be as capable as they are of rapidly overrunning new areas into which they have been accidentally introduced.

DARWIN'S VIEWS.

Those who oppose the theory of simultaneous development seem to assume that they have the authority of Darwin on their side. This is by no means clear. What Darwin's opinions were on the points we

have been discussing are not so easily ascertained as might be supposed, owing probably to the fact that Darwin's views underwent a change, which he himself admits, between the times of the publication of the first and last edition of the Origin of Species.

I. Thus with respect to the descent of all individuals of the same species from a single ancestor or pair of ancestors, we have the following passages (sixth edition) :

(p. 320). . . . "Individuals of the same species. . . . . must have proceeded from one spot where their parents were first produced."

(p. 259) "If we bear in mind. . . . how often a species may have ranged continuously over a wide area and then become extinct in the intermediate tracts, the difficulty is not insuperable in believing that all the individuals of the same species are derived from common parents."

(p. 406) "All individuals of the same species. . . . are descended from common parents."

On the other hand we have the following passage :

(p. 322). . . . "Individuals of the same species inhabiting the same area will be kept nearly uniform by intercrossing ; so that many individuals will go on simultaneously changing, and the whole amount of modification at each stage will not be due to descent from a single parent."

This last passage, though somewhat ambiguous, leaves us in some doubt whether Darwin held that species are to be traced back to a single ancestor or ancestral couple, but they leave no doubt but that he was of opinion that each species originated in a single locality from which it spread by migration.

II. On the closely allied point as to the existence in the individuals of species of a tendency to parallel variations there is a somewhat similar though less uncertainty as to Darwin's final opinion.

Thus, on page 125, he heads a paragraph with the words :

Distinct species present analogous variations, so that a variety of one species often assumes a character proper to an allied species.

This he illustrates by the frequent occurrence in one breed of domestic pigeons of a characteristic of another breed, but not found among the aboriginal rock pigeons from which both have descended, as for example the occurrence of fourteen or sixteen tail feathers in

the pouter, thus assimilating it in this respect to the fantail, and he adds the words :

> "I presume that no one will doubt that all such analogous variations are due to the several races of pigeons having inherited from a common parent the same constitution and *tendency to variation*, when acted on by similar unknown influences."

And after mentioning other instances of analogous variation he goes on to add :

> "According to the ordinary view of each species having been independently created we should have to attribute this similarity . . . . not to the *vera causa* of community of descent and a *consequent tendency to vary in a like manner*, but to three separate yet closely related acts of creation."

Again on page 127 we find the passage :

> "The difficulty of distinguishing variable species is largely due to the varieties, mocking as it were other species of the same genus" . . . .
>
> "But the best evidence of analogous variation is afforded by parts or organs which are generally constant in character, but which occasionally vary so as to resemble in some degree the same part or organ in an allied species. I have collected a long list of such cases."

Now it is obvious that if the individuals of allied species shew this tendency to parallel variation such tendency must be far stronger among individuals of the same species and still more among individuals of the same variety, and accordingly on page 91 Darwin says :

> "The tendency to variability is itself hereditary, consequently they" (*i.e.* the individuals belonging to varieties of a species) "will likewise tend to vary, and commonly in the same manner as did their parents."

These passages seem to prove almost conclusively that Darwin held that varying species have a tendency to vary in particular lines, determined by those lines of variation which have evolved the species itself.

On the other hand, however, there are passages in which Darwin seems explicitly to deny the existence of a general tendency to simultaneous variation.

Thus on page 319 we read :

> " There is no evidence, as was remarked in the last chapter, of the existence of any law of necessary development."

And on page 291 :

> " These several facts accord well with our theory which includes no fixed law of development, causing all the inhabitants of an area to change abruptly or simultaneously, or to an equal degree."

If, however, these passages be carefully read with the context it will appear that the law of development here denied is not one affecting the individuals of particular species only but the individuals of all species inhabiting a given area.

Nevertheless the following passage seems to shew that at the time of the publication of the first and earlier editions Darwin believed that species did originate from the casual variation of an individual, and that it was only after the year 1867 that he came to the conclusion that new permanent varieties and consequently new species could only be formed by similar and simultaneous variation in many individuals of the same species.

In the IVth Chapter of the 6th edition, page 71, after illustrating the action of natural selection by the example of the swiftest and slimmest wolves alone being able under certain circumstances to subsist and propagate their race, he goes on to say :

> " It should be observed that in the above illustration I speak of the slimmest individual wolves, and not of any single strongly marked variation being preserved. In former editions of this work I sometimes spoke as if the latter alternative had frequently occurred " . . . " until reading an able and valuable article in the North British Review 1867 I did not appreciate how rarely single variations whether slight or strongly marked could be perpetuated" . . . .
>
> " It should not . . . . be overlooked that certain rather strongly marked variations, which no one would rank as mere individual differences, frequently occur owing to a similar organization being similarly acted on." . . . .
>
> " *There can be little doubt but that the tendency to vary in the same manner has often been so strong that all the individuals of the same species have been similarly modified without the aid of any form of selection.*"

These passages would naturally lead to the conclusion that Darwin originally believed that varieties and species originated from some casual change in an individual, but subsequently came to the conclusion that varieties and species could only establish themselves by the simultaneous like variations of many. There is however a passage in the sixth edition of the "Origin of Species," which occurring as it does in the final summary (p. 423) seems to render this conclusion again somewhat doubtful.

Darwin is treating of the theory of the descent of organic life from an individual primordial form.

> "It has, he writes, been maintained by several authors that it is as easy to believe in the creation of a million beings as of one, but Maupertuis' philosophical axiom of "least action" leads the mind more willingly to admit the smaller number; and certainly we ought not to believe that innumerable beings within each great class have been created with plain but deceptive marks of descent from a single parent."

Here clearly Darwin regards the possession of similar characteristics as conclusively proving descent from the same ancestor.

The balance of evidence seems, however, on the whole to be in favour of the conclusion that as far as existing species are concerned, Darwin held that they have been evolved subject to selection, by the tendency among individuals of pre-existing species to simultaneous variation in some limited number of directions determined by a subjective cause which may be termed hereditary influence.

If then we recollect that Darwin held (p. 42) that wide-ranging, much-diffused and common species vary most, it seems almost impossible to avoid the conclusion that according to Darwin's theory new variations in the same direction will occur over the area or in the different areas over which the species is diffused and will develope into like specific forms wherever surrounding circumstances are not unfavourable, whether it be in one locality or many.

In confirmation of this view we may quote the following passage (Origin of Species, p. 419):

> "The existence of closely allied or representative species in any two areas implies on the theory of descent with modification that the same parent forms previously inhabited both areas, and we almost invariably find that whenever many closely allied species inhabit two areas some identical species are common to both."

Darwin himself explains this by supposing that of the identical species formerly occupying these two areas, some have varied and some have not, those which have varied being now allied species, those which have not varied being identical species.

But as according to him widely diffused species are varying species, it seems more in accordance with probability to hold that the closely allied species are those which have varied from the common parent in slightly different directions in the two areas, while the identical species are those which have varied still more closely. At any rate it seems impossible to deny that simultaneous parallel variation is a *vera causa* for the existence of widely diffused species.

ORIGIN OF LIFE.

Let us now examine whether Darwin's theory as to the origin of organic life on the earth throws any light on his views as to subjective variational tendencies.

In the first edition of the "Origin of Species," page 484, we find the following passages :

> "I believe that animals have descended from at most only four or five progenitors, and plants from an equal or lesser number."
>
> * * * * * *
>
> "I should infer from analogy that probably all the organic beings which have ever lived on this earth have descended from some one primordial form, into which life was first breathed."

In the sixth edition, pages 424 and 425, similar passages occur, only that for the second passage the words *into which life was first breathed* are omitted.

Are we to understand from this that Darwin regarded the origin of organic life on the earth as supernatural? If the words "into which life was first breathed" which appear in the first edition seem to imply the attribution of a supernatural origin to organic life, their omission in the sixth edition might be regarded as showing a desire on Darwin's part not to commit himself to any such theory. On the other hand, however, the following passage, also from the same edition, seems to point to a different conclusion. On page 423 we find the words :

> "It has been maintained by several authors that it is as easy to believe in the creation of a million beings as of one, but Maupertuis' philosophical axiom of 'least action' leads the mind more willingly to admit the smaller number."

These words seem to be only consistent with the well-known theological doctrine of "economy of miracles" and one would be inclined to conclude from them that Darwin considered that origin of organic life on the earth was supernatural.

The mere fact moreover of Darwin tracing back all organic life to one or at the most a dozen primordial progenitors seems inconsistent with any hypothesis but that of his believing in the supernatural origin of life. If life originated from inorganic matter according to a natural law it is quite incredible that this law in all earthly time and space should have operated only once or even at most a dozen times.

Assuming then that Darwin regarded the origin of organic life on the earth as supernatural, it is obviously in vain to look to his views in this respect for any light it may throw on the origin of species.

But though Darwin's views cannot assist us in this respect, it may be of some use to consider the subject on its own merits.

Setting aside the theory of supernatural origin of life, not as being untrue or improbable but as putting the question out of the reach of scientific enquiry, there are three other hypotheses on this subject which seem worth consideration:

(1.) Organic life always existed on the earth.

(2.) Organic life came to the earth by migration.

(3.) Organic life originated and perhaps still continues to originate according to some natural law.

Let us consider each of these hypotheses in turn.

(*1.*) *Organic Life always existed on the Earth.*

Against the first hypothesis it may be urged:

I. The perpetual existence of life on the Earth is inconceivable Everything must have had a beginning, therefore organic life on the Earth must have had a beginning.

II. The perpetual existence of organic life on the Earth is inconsistent with La Place's theory of the origin of our planetary system, according to which theory the Earth must at one time have been in a physical condition inconsistent with the existence of organic life in any form known to us or from which it is at all probable that existing life would have been derived.

I. To the first of these objections the reply is that the word inconceivable is here misused. No doubt the forms of things as we observe them are always changing, and in this sense we are constrained to believe that all that exists is transitory. On the other hand, however, we are equally constrained to believe in the continuity

of the present with the past, and we cannot realize a time when nothing existed and something came out of this nothing. An infinite period of vacuity ending with a creation is in the highest degree incredible, and if we are precluded from ascribing a beginning to the universe as a whole we cannot be compelled to ascribe a beginning to organic life either generally or on the Earth.

II. As to La Place's theory it must be remembered that it is only a theory, and that recent discoveries in Astronomy and Geology have not rendered it more plausible.

Geology points to changes in the Earth's temperature, but certainly not to continuous change in one direction. If in the Arctic regions there are indications of there having been a higher average temperature in certain remote geological periods, there are no less clear indications of a lower temperature having prevailed in regions now temperate or even tropical. Geology moreover claims for the formation of the Earth's crust as known to us periods of time which the physicist arguing from La Place's theory will not for a moment allow.

So also in Astronomy the observed motions of some of the Asteroids and Satellites are admittedly at variance with the truth of La Place's theory.

On the whole therefore it would seem that the theory of the eternal existence of organic life on the Earth is not one that is to be summarily rejected as inconceivable or impossible, at the same time it is obvious that this theory is incapable of throwing any light on the laws of evolution, as might indeed be concluded from the consideration that what we call infinite is negative or at least privative and not positive.

(*2.*) *Organic Life came to the Earth by Migration.*

I. Some seem to think that this hypothesis is altogether unworthy of consideration, and it must be admitted that it is very difficult to understand how any form of organic life that we are acquainted with can have been brought to the earth from without. It would perhaps, however, be too much to say that such a thing is impossible, though we may safely conclude that if it has happened it is in the highest degree improbable that it has happened only a few times. We are perhaps too ready in placing the boundary of the possible at the limits of our perceptions. We forget that in the minutest particle of meteoric dust there is, in a sense, as much room as there is in a planet, and that we have not the least reason to suppose that organic forms are limited as

to size by the perceptions of our senses, even though aided by instruments a million times more powerful than the best devised by man. It is as well to remember that the abysses of the infinitely little are really as profound as those of the infinitely great, that if our telescopes reveal to us ever-increasing profoundities of distance, our microscopes equally reveal to us the profoundities of minuteness, and in neither direction do we find trace nor can we conceive any possibility of limit.

II. The migrationary hypothesis may likewise be objected to on the ground that it merely removes the problem as to the origin of organic forms to some other time and place. This is true, but the hypothesis of the co-eternal existence of organic and inorganic forms in the universe as a whole does seem to present fewer difficulties than their co-eternal existence on the Earth.

Moreover we are not now concerned with the relative probability of the various hypotheses as to the origin of organic life, but with the light which these hypotheses respectively throw on the theory of subjective tendency to variation. Whether the migrationary hypothesis is or is not satisfactory as to the origin of life, all we are concerned about is whether this hypothesis supposes the introduction of life on the earth in one or only a few cases, or in an indefinite number of cases, and it can hardly be denied that the latter is the case.

(*3.*) *Organic Life originated and perhaps still originates from Inorganic Life according to some Natural Law of Continuity.*

This hypothesis, sometimes called that of Spontaneous Generation is summarily rejected by some as being inconceivable. Here again the word inconceivable is wrongly used. There can be no difficulty in the conception of continuity between what is called the organic and the inorganic forms of matter, though there may be some difficulty in believing that such continuity actually exists, since all our experience goes to shew that organic forms invariably proceed from previously existing organic forms. This difficulty would, however, be somewhat diminished by the reflection that our experience imperfectly covers only an infinitesimal part of nature. Because physicists have not yet consciously succeeded in producing organic from inorganic forms in the glass bottles of their laboratories, it by no means follows that in the wonderfully diversified conditions which exist and have existed on the Earth, such production is

impossible. As already observed, in the boundless field of the infinitely little there are infinite possibilities. The doctrine of spontaneous generation has never been disproved, and probably never can be disproved, though the too hasty conclusions of some physicists who have claimed to produce the necessary conditions for the passage from the inorganic to the organic forms have been successfully disproved.

To summarise these results, it may be said that the hypotheses of the supernatural creation of organic life, or of its perpetual existence on the Earth, throw no light on its subsequent evolution, but the hypotheses of the introduction of organic life by migration or spontaneous generation seem inevitably to lead to the conclusion that life has originated on the Earth, not as Darwin supposes from one or a dozen ancestors, but that the process of life introduction is one that is continually going on, or at any rate must have happened a countless number of times. Whether the *forms* of life thus introduced are countless is another question. If it be permitted us to reason about matter so entirely beyond our experience, we might infer from analogy that the process of what may be called organic crystallization is possible in only a limited number of ways so that the number of forms produced by spontaneous generation, if such generation exists, would be comparatively few. If the migration theory be true, then on the other hand there would seem to be no limit to the number of forms that might be introduced.

If the migration theory be maintained then the Darwinian hypothesis may be sufficient by itself to account for the present state of terrestrial organic life, since the immigrant forms may have brought with them the hereditary tendency to vary in specific lines.

If, however, the doctrine of spontaneous generation be maintained, then we seem compelled to supplement Darwin's hereditary tendency to specific variation by Lamark's theory of a purely subjective tendency to specific variations, supplemented by Darwin's theory as to the objective control of such subjective tendency ; but, whether we accept Darwin's theory alone or use it as a supplement to Lamark's, whether on account of heredity or on account of heredity and subjective law combined, we are driven to the conclusion that *the tendency is to variation in specific directions, and consequently to the independent origination of the same varieties, and therefore of the same species and genera, so that independent simultaneous variation must be admitted as a vera causa for the existence of widespread species.*

NOTE ON TEETH OF THE ZIPHIOID WHALE, *MESOPLODON LAYARDII* (GRAY), EXHIBITED AT THE MEETING OF THE SOUTH AFRICAN PHILOSOPHICAL SOCIETY,

On Wednesday, August 29, 1888,

By Roland Trimen, F.R.S., &c., Curator of the South African Museum.

As long ago as 1865, I made for my predecessor, Mr. E. L. Layard, outline drawings of the heads of two different species of Ziphioid Whales then in the South African Museum; and he sent these drawings to the late Dr. J. E. Gray, at that time Keeper of the Zoological Department of the British Museum, who had bestowed much study on the Cetacea. Dr. Gray determined the two whales in question, merely from the drawings and Mr. Layard's notes, to be both new species, and described them in the Proceedings of the Zoological Society of London for 1865 respectively as *Ziphius Layardii* and *Petrorhynchus Capensis*. The latter has since been identified with *Ziphius indicus*, Van Beneden; but the former (which in the form and development of the teeth is by far the more remarkable) retains its position as a very distinct species, peculiar, so far as known, to Cape seas, and is placed by Prof. Flower (Trans. Zool. Soc. Lond., 1872, vol. viii., p. 211) in Gervais' genus *Mesoplodon*.

The Ziphioid Whales are most nearly related to the Cachalots or Sperm Whales (as is shown in Prof. Flower's memoir just quoted), but, amongst other distinctions, differ remarkably in the very reduced condition of their teeth, which (confined to the lower jaw) are rudimentary and concealed in the gum, with the exception of one pair (or occasioually two pairs) at or towards the extremity. The whales of this group were evidently numerous in Tertiary times, as their abundant remains in the Crag formation testify; but at the present time they are certainly rare, only individual (usually stranded) specimens now and then occurring in various parts of the world.

Mesoplodon Layardii exhibits an extraordinary development of the only two teeth it possesses. They are situated at some little

distance behind the apex of the mandible, are greatly compressed laterally (so as to resemble a thick strap), and slant considerably backward, while they are sufficiently long and curved inward superiorly as to meet, or almost to meet, above the long and narrow snout or beak. At the tip of each of these singular tusks there is in front a conical compressed projection, looking like a small tooth artificially inserted; this Prof. Moseley (*Notes by a Naturalist on the "Challenger,"* 1879, p. 158) regards as the original small cap of dentine of the tooth of the young animal, which, without increasing in size, is carried up by the apparently abnormal growth of the fang, the latter constituting the bulk of the tusk.

Both Mr. Layard and myself were at once struck with the obvious difficulty that, if this singular position and form of the tusks were not due to an individual aberration or monstrous growth, the case was one of a great mammal with its jaws naturally so locked together as to be unable to open its mouth for more than a very little distance. Both Dr. Gray and Professor Owen were inclined to look upon the single original specimen as shewing merely an individual malformation; but, as Prof. Flower has recorded, Mr. Layard possessed a single tooth of another individual having an exactly similar conformation, and the discovery by Prof. Moseley (while here in 1873 on the Cruise of the "Challenger") of the lower jaw, with quite similar tusks, of a third example rendered it almost indisputable that the case was one of normal occurrence in this species. It is not known whether, as Prof. Flower suggests, the tusks are peculiar to the male animal. In connection with the difficulty referred to, it occurred to me in 1865, when examining the type specimen, that possibly the flattened tusks (which even *in situ* on the skull showed some elasticity in yielding and separating when the lower jaw was pressed downward) were to some extent movable at the will of the animal; and I have found, in the account by the late Sir Julius Haast of the capture of an allied Ziphioid Whale (*Berardius Arnousi*) near Canterbury in New Zealand, that an eye-witness of the dying struggles of this stranded specimen observed its front teeth to be movable and protrusible. The food of these Cetaceans, as far as ascertained, consists, both in the Northern and Southern hemispheres, of *Octopus* and allied cuttle-fish; and, if the mouth of *Mesoplodon Layardii* is as closely locked by the over-arching tusks as it appears to be, it is difficult to understand how it can capture these active and watchful cephalopods.

The tusks of this whale which I have brought for exhibition are unquestionably a natural pair ; agreeing very exactly in size, outline, and curvature. I pur hased them for the South African Museum, which since the original skull was sent to the British Museum, had possessed no specimens representing the species. The only particulars I could obtain from the vendor as to their history were to the effect that they were received for sale, among various other "curios," from Knysna on the South Coast of the Colony.

I do not think that the actual locality of the type specimen is on record. Prof. Moseley's two examples were captured at Simon's Bay and Walwich Bay respectively ; the former was stated to be about 10 feet in length, and the latter from 16 to 18 feet.

An animal of such rarity and such exceptional dentition, and apparently peculiar to South African seas, is of great and special interest ; and I trust that, by calling the attention of members of the Society to what is known of the species, further specimens and information may ere long be discovered.

ON SOME PHOTOGRAPHS OF LIGHTNING FLASHES.

By C. Ray Woods.

[Read 29th August, 1888.]

The photographing of lightning flashes, it may be stated at the outset, is no new thing. Many successful photos of flashes have been obtained since the introduction of the Rapid Dry Plate Process now so universally used, and some of these, having come under the notice of the Royal Meteorological Society, the Society deemed the matter of so much importance that a Thunderstorm Committee was formed last year for the purpose of collecting and reporting on such photographs. Circulars were sent out inviting specimens, with a request to all amateur and professional photographers to assist the Committee. The process is simple. A camera focussed for long distance objects is pointed in the direction whence the flashes proceed, the carrier slide is withdrawn and the lens uncovered for a time. It is advisable to remain on the watch and cover the lens after the appearance of a flash in the field of view, and then to expose a fresh plate. A series of flashes on one plate, though it might make a more effective picture, would not lend itself so readily to scientific investigation.

On the evening of July 30th last, I took advantage of the thunderstorm that then occurred to expose a series of plates. The most open view from my residence is in the direction of the Cape Flats, and in that direction bright flashes, or sparks, to use a better term, were frequently appearing. Care was taken to get only one flash on each plate, with the result that the larger proportion were failures, but on several plates I obtained images which I pass round. Though inferior and disappointing compared with the others to which I draw your attention, such as this photo obtained by Mr. Hawksworth in Natal, and particularly this fine photo obtained in Cape Town by Mr. Allis about a twelvemonth ago, they are of some interest, I venture to hope, in elucidating and amending the report just issued by the Thunderstorm Committee already referred to. I wish particularly to draw attention to the fact that the sky was almost covered with clouds of varying thickness; this not only rendered some of the

sparks too feeble to impress themselves on the plate, but cut off parts of others, thinned their images in some places, and it may be assumed with considerable certainty showed that the images that were obtained were only such parts of the flashes or sparks as were visible through and between the cloud masses. Before referring to their main features let me draw attention briefly to the recent report of the Meteorological Society's Committee, drawn up by the Hon. Ralph Abercromby.

The Committee reports that from the consideration of sixty photographs sent from Europe and America in answer to their circular, "it is evident that lightning assumes various typical forms, under conditions which are at present unknown." The following appear to be some of the most typical forms :

1. *Stream Lightning*, or a plain broad, rather smooth streak of light. Only two or three specimens of this form were received by the Committee.

2. "*Sinuous Lightning*, when the flash keeps in some one general direction, but the line is sinuous, bending from side to side in a very irregular manner. This is by far the commonest type." It is very noticeable that the thickness of the line varies during the course of discharge. Sometimes the thinnest part is the highest, at other times a flash in the air begins thin, broadens out in the middle and thins again on approaching the earth. The Committee says that it can offer no explanation of this at present, but draws attention to the fact in some photographs of electric sparks taken from an induction coil, those of high tension are thinner than those of low tension. This, I venture to think, affords no explanation of variation in intensity during the course of one flash, though it may explain the variation in thickness between two flashes.

3. *Ramified Lightning*, in which part of the flashes appears to branch off from or run into the main streak like the fibres from the root of a tree.

4. *Meandering Lightning* ; sometimes the flash appears to meander about in the air without any definite course and forms small irregular loops. The thickness of the same flash may vary considerably in different parts of the course, as above mentioned.

5. *Beaded or Chapletted Lightning* ; sometimes a series of bright beads appear in the general white streak on the photograph. Occasionally these brighter spots appear to coincide with bends in a meandering type ; but often the beads appear without any evident

looping of the flash. The Committee goes on to point out, what is also plainly evident in the beaded appearance in the photographs which I have taken myself, that it is evidently due to the flash taking at times a course to and from the observer and giving a longer exposure to these spots.

6. *Ribbon Lightning.* "Nearly one-sixth of the photos received by the Society show flashes exhibiting more or less of a ribbonlike form. One edge of the ribbon is usually much whiter and firmer than the other." The Committee points out that this form is probably due to double image formed by internal reflections of doublet lenses. This defect then, if it is only a defect, is visible in my own photos, more particularly in those parts of the flash nearest the edges of the plate. My experiments were made with a doublet lens, as at the time they were taken the report I quote was not published and I was not aware that a single lens gave the best results.

Of these various forms I pass round illustration and draw attention to the diagrams on the wall which are roughly copied from specimens issued with the report.

The beaded appearance in my plates I have already drawn attention to. Apart from this they might be classed as sinuous, meandering, or as ramified, as they partake more or less of all three characters. I have already mentioned the clouded condition of the sky and I need not add that clouds must necessarily be present where lightning discharges take place. The Committee have singularly enough overlooked this, the simplest explanation of the variation in thickness of a flash. Cannot the cutting off of parts of a flash by cloud, and modification of the other parts of absorption through cloud, explain some of these forms assumed by lightning flashes ?

Let me draw your attention to two diagrams roughly copied from photographs of the electric spark taken by Mr. Trueman Wood, of the Society of Arts, with a large induction coil.

Here is a single spark which obviously corresponds with Class 1 or Stream Lightning. The next is the result of six successive sparks impressed on the plate. By covering portions of this photograph we can get tolerably near approaches to the sinuous, ramified, and meandering types. The beaded type, which is readily explained, has been obtained by Mr. Wimshurst with a large electrical influence machine.

In a thunderstorm, as terminals of the natural electric machine we have not one only but sometimes many points of discharge, in the

irregular form of the clouds, when it is from cloud to cloud, and also when from cloud to earth, the earth may present various points also, rather than simply one terminal. The discharge, whether in many simultaneous or many successive sparks must necessarily be very complicated, but still of the character shown by the six sparks in the diagram. The masses of cloud obstructing parts of this complication of sparks appear to my mind to afford sufficient explanation of the various forms of lightning shown in the photographs.

To sum up. It appears probable then, that so far from lightning assuming various forms under conditions at present unknown, it only assumes two forms such as may be obtained in the laboratory, the single spark and the compound flash or series of sparks; the various forms of the latter being due to modification in appearance by the obstruction of cloud masses. Bearing in mind that the former view is put forward by one of our best living representatives of Meteorological Science, I put forward a contrary view with all due diffidence, but in apology for my presumption I lay stress on the fact that my own view is based on photographs taken by myself coupled with personal observation of the condition of sky at the time, whereas the view taken in the Meteorological Society's report is based on an examination of photos taken by others. In their circular the Meteorological Society only asked for the photographs. Had they ventured to ask photographers for particulars, were it only a general description of the sky, though they might have found fewer correspondents to communicate with them, I venture to think that the Committee would have adopted views similar to my own.

Photographers in other parts of the Colony have better opportunities of recording thunderstorm phenomena than we have here. If their attention is drawn to the fact that by a little trouble on their part they may get results not only interesting in themselves, but of importance in investigating phenomena which cannot be studied without the use of the camera, they will doubtless join in furthering the enquiry.

I may point out in conclusion that in no photos that have yet been obtained do we get anything like the ridiculous conventional zigzags which artists represent in their pictures.

SYNOPSIS.

1. Epitomised report of Thunderstorm Committee of Meteorological Society of London on certain sixty lightning photos and their opinion that lightning tends to assume various forms under conditions at present unknown.

2. That the Meteorological Society have overlooked the modifying influence of cloud in obstructing and absorbing parts of flash, although it explains most simply one matter that puzzled them.

3. Enquiry whether the modifying influence of cloud cannot also explain the various forms which the Meteorological Society has classified.

4. The greater probability that lightning assumes only certain forms which can be obtained under simple conditions in the laboratory in contradistinction to the view put forward by the Meteorological Society of London.

PHOTOGRAPHS OF LIGHTNING FLASHES TAKEN SEPTEMBER 18, 1888.

By C. Ray Woods.

[Read 26th September, 1888.]

At the meeting held here a month ago I brought forward some attempts made to photograph lightning flashes during the thunderstorm of July 30th. I on that occasion drew attention to the report issued by the Thunderstorm Committee of the Royal Meteorological Society with various illustrations from the "Photo News" and by photographers in this Colony. A consideration of the various photos of lightning flashes that had passed through my hands led me to draw a different conclusion to that given in the report, which said that there was reason to believe that "lightning assumes various forms under conditions at present unknown." The view that I ventured to put forward was that the conditions did not greatly differ from experiments in high tension electricity made in the laboratory; that it is more probable that lightning only assumes two forms, the single spark and the compound flash or series of sparks, the various forms of the latter being due to modification in appearance only by the obstruction of cloud masses.

The photographs taken on the 18th do not tend in any way to modify those views. Apart from the point brought forward in my last part, I shall merely content myself with a brief description of the photos. It would be interesting to speculate on the physical, chiefly electrical, conditions of this and the preceding storm, but as any evidence that may be derived from these photos is too meagre for any theorizing beyond vague suppositions, and unless any points are brought forward in discussion to cause me to express any vague ideas I may have formed thereon, I prefer to postpone the matter till I can speak with greater confidence. By the time the next thunderstorm occurs, I hope to be in a position to record it on a more extensive scale.

THE STAR LORE OF THE SOUTH AFRICAN NATIVES.

By W. Hammond Tooke.

[Read 31st October, 1888.]

It is with much diffidence that the writer places the following few notes upon some of the conceptions held by the barbarous tribes peopling South Africa respecting the heavenly bodies before the members of this Society.

The facts are too few and fragmentary to justify any attempts to formulate a theory on them, nor has any such attempt been made. They are merely recorded here for the sake of comparison.

It may be held that they are not of any practical interest; but such as they are, it is from these almost trivial data that powerful side lights are sometimes thrown upon what would otherwise be completely buried in the mists of obscurity, namely the unwritten history of the races, some prolific, some nearing extermination, which preceded the European occupation of this continent. And they help us to realise a mental condition which has much to teach us, both by its resemblances to our own and by its dissimilarities.

"It is," says Professor Drummond, "a wonderful thing to look at this weird world of human beings, half animal, half children, wholly savage and wholly heathen . . . It is an education to see this sight, an education in the meaning and history of man. It is to have watched the dawn of evolution. It is to have the great moral and social problems of life, of anthropology, of ethnology and even of theology, brought home to the imagination in the most new and startling light."

BUSHMAN.

Two of the most remarkable characteristics of the South African Bushman, that "unglückseliges Kind des Augenblicks," apparently the most degraded type of humanity existing, are (1) his pictorial talent and (2) his acquaintance with the "starry heavens above us." The Homeric Greek has not done more towards distinguishing the Stars of the northern hemisphere than have the half-starved outcasts of the Kalahari Desert or the cave-dwellers of the Drakensberg in respect of those spangling our southern skies.

Miss Clerke has shown in an interesting paper contributed to *Nature*, how Hesperus and Phosphorus, Orion and Arktos, the Pleiads and Hyads nearly complete the list of stars distinguished by name in the Homeric poems from the rest of the "stellar multitude." The Bushman can shew us a longer list than this.

Thus, we learn from Dr. Bleek, that the Pointers of the Southern Cross are called the Male Lions; α, β, γ, Crucis are the Lionesses. Aldebaran is the Male Hartebeest, α Orionis the Female Hartebeest; the Male Eland is Procyon, his wives are Castor and Pollux, the Magellanic clouds are Steinboks, the Sword and the Stars in the Belt of Orion are Tortoises, &c., &c.

Both Greek and Bushman dated their seasons by the annual rising of the Stars; but, while the Greek pictured groups in the sky with which he connected legends of god and heroes the Bushman traced no constellations. He named the single Stars which he saw at certain periods from the animals or other objects which at such time come into season or became more abundant. Thus Canopus or the "Ant egg" star by its appearance shows that this article of food might now be sought for.

The Bushmen classify the Stars into night stars and dawn stars. Jupiter is known as the "Dawn's Heart," and Leonis is her child.

The following legends profess to describe the origin of some of the heavenly bodies:

The Milky Way was formed by a girl of the ancient race throwing ashes into the sky.

The Sun was a man formerly living on the earth from whose armpits light proceeded. It, however, only illuminated a small space round his house; and while he was asleep some children of the ancient Bushmen were sent to throw him into the sky "since when he shines over the whole earth."

The Moon was caused by the Mantis or chief Bushman deity throwing his shoe into the sky, ordering it to become the Moon and disperse a darkness which was caused by the bursting of the eland's gall bladder. The Moon is red because the shoe of the Mantis was covered with the red dust of Bushmanland.

But according to another legend our satellite was a man who had incurred the wrath of the Sun. The Sun attacked him with his knife, *i.e.*, his rays, until the Moon was by degrees cut away, all but a little piece, which the Moon implored him to spare for his children. He then began to increase again until he reached his original size, when the process was repeated.

KHOI-KHOI.

From the evidence that remains to us of Hottentot life, we gather that the Hessequa, Cachoqua or Namaqua resembled their pygmy neighbours and foes in taking a lively interest in matters astronomical. Nay more, they regarded some of the heavenly bodies with a veneration approaching worship.

Thus Dapper tells us of the tribes within the vicinity of the Dutch Settlement that they had some superstition regarding the New Moon, at the appearance of which they crowded together making merry the whole night dancing and singing. Nieuhof says that they specially honoured the Moon and probably did not hold the Sun in less esteem. Leguat believed that they paid the Moon some kind of worship. Kolb states that both at New and Full Moon they continued dancing and singing throughout the night; and Captain Cowley calls them "worshippers of Dame Luna."

With regard to the tribes further north we learn from the commander of an expedition sent into Great Namaqualand in 1761 by Governor Tulbagh that the Namaqua religion then consisted in worshipping and praising the New Moon.

The Nama term for "moon" is *Khab*, in Kora *Kham* "the returner." The present antiquated and obliterated nature of this name (so Dr. Theophilus Hahn concludes) points to its early application to our satellite, before the separation of the Hottentot or Khoi-Khoi clans.

Kolb speaks of it as being the "Sichtbar Gott" whereas *Ticquoa* was the "*un*sichtbar Gott." Nama legends appear to connect the moon *Khab* with another being *Heitsi-eibib*. And according to Dr. Hahn *Ticquoa* or *Tsui* ǁ*Goab*, a word now always applied to the Supreme Being, originally signified Red morning or Dawn, and is also synonymous with *Heitsi-Eibib*, "the Dawn Tree." Therefore, says Dr. Hahn, it can no longer be doubtful that "the Moon is identical with *Tsui* ǁ*Goab* as the 'Lord of Light and Life.'"

It at all events seems made out that the Hottentot conceptions of the Supreme Being can be identified with the Light or Sky in its different manifestations of the oft-recurring moonlight or victorious dawn.

To turn to the Stars: α and β Centauri are called *mura* "the two eyes," μ 1 and 2 in the Scorpion are the "eyes of the lion." One of the names for Venus is "the man with the fingers cut off."

The Pleiads are called *Khunuseti*, which means offshoots or the stars of the offshoots, namely the "Stars of the budding season or

spring," another meaning given to them is "those who spring or shoot off from one stem, a cluster." Both these meanings will be found in various Bantu dialects as will be seen below.

The *Khunuseti* were daughters of *Tsui* ‖*Goab*, the Dawn or Sky God. It is related of them that they once said to their husband, "Go and shoot those three Zebras for us, but if you fail to shoot, do not return!" The husband went with one arrow, and shot with his bow. He did not hit and sat because his arrow had missed the Zebras. On the other side sat the Lion, and watched the Zebras and prevented the man from picking up his arrow, and because his wives had cursed him he could not return, and then he sat in the cold night shivering and suffering from thirst and hunger.

And the *Khunuseti* said to the other men: "Ye men, do you think that you can compare yourselves to us and to our equals? There now! We defy our husband to come home because he has not killed game."

This legend has been pictured in the Sky by the Namaqua. The husband of the *Khunuseti* or Pleiads is Aldebaran; his bow is π, π'^2 π^3 Orionis; his arrow three other stars in the same constellation, probably the sword; his sandals are ε and δ of the Hyades, his kaross is ϑ and γ Hyadum; the Zebras are δ'^2 and ζ Orionis, or the three Stars in the Belt and the Lion is α Orionis (Betelgueuse) or in another version Sirius.

This fact would seem to show that the Hottentot differs from the Bushman in his method of naming the Stars; not the period of their appearance and its connection with the seasons seem to have suggested the names given to the stars above mentioned, but their relative position towards each other by which they illustrate the legend.

This tradition, we may remark in passing, appears to show that in early times the Hottentot women had enlightened views as to the position of their sex.

BANTU.

Far different to the conceptions entertained by the yellow races of South Africa of the heavenly bodies are the notions held respecting them by the dark skinned Bantu.

The Kaffir knows and names Sun, Moon and Star, but he has no legends as to their creation. "These things," to adopt Tiyo Soga's words, "seem to have baffled his imagination." With few exceptions, such as the Morning and Evening Star and the Pleiads, he has no distinctive epithet for the more conspicuous stars. He never lifts

his eyes to trace the patterns on "this majestical roof, fretted with golden fire," nor to note the rising or setting of planet or asteroid, except as it may serve to mark the advent of the planting season, &c.

The ama Zulu and ama Xosa call the Sun *Ilanga*, but they seem to regard it with no veneration and look on it neither as an ancestor nor as a deity.

The Xosas have indeed a tradition that the creator or first ancestor, *Uhlanga*, came out of a cave situated *eastward* "from whence the Sun issues every morning," and this legend together with the verbal similarity of *Uhlanga* to *Ilanga* might lead to the conjecture that there is a common origin on the two words. And doubtless both words bear within them the original conception of rising "originating from." *Uhlanga* is however more closely connected with *Umhlanga*, a reed, and the most common form of the myth is that *Uhlanga* the first man sprang from a reed or bed of reeds. Thus it would rather seem to indicate that the Kaffir at some early period migrated from some marshy locality like the shores of Lake Bangweolo or the ambatchchoked head waters of the Nile than that he ever considered himself a Child of the Sun.

The use of the word *Ilanga* for Sun seems confined to the Zulu and Xosa tribes. The ama Mbalu or ama Langa have disused it on account of a noted chief of their clan bearing the name and they have substituted *isota* instead.

The word most universally used among the Bantu is some form of *Zuwa*. Thus :

Tribe.		Locality.		
Ama Tonga	...	Sofala ...	...	Zuba
Ba Nyai	...	Near Tette ...	...	Zuwa
Ma Ravi	...	South of Lake Nyassa	...	Dsua
Wa Sambara		... ...	...	Zua
Wa Swaheli	...	Zanzibar and Mombasa	...	Jua
Wa Taveita Wa Chaga	}	Kilimanjaro ...	{	Zuwa Eruwa
Wa Ganda	...	L. Victoria Nyanza	...	Nguba
Eshi Kongo	...	Sao Salvador ...	...	Ntuva
Ora Herero	...	Damaraland ...	...	Eyuva.

Only among the Wa Chaga does this word connote the meaning of God or deity as well as Sun, and this may perhaps be explained by their proximity to the Masai or other Sudan tribes amongst which heaven worship exists.

As with *ilanga* (Sun) so it may be said of *inyanga*, the Kafir and Zulu word for Moon, that it is rarely found in any other Bantu language.*

Inyanga appeared to Dr. Bleek to be of Hottentot origin, and if so this would show perhaps that the veneration paid by the Khoi-Khoi to the Moon had had the effect on the neighbouring Kafir of inducing him to adopt the Hottentot word. No sign of Moon worship, however, has been observed in the Kaffir, who selects a full moon for his festivities for the same reason that we choose it for a moonlight walk round the Kloof.

Two words are most commonly applied by the Bantu to the Moon of which it may be said, speaking roughly, that one is in use on the West and one on the East Coast.

The Zulu for moonlight is *unyezi*, probably from an Archaic form *inkwezi* (see Bleek), and we find this word applied to the Moon among the following tribes on the East Coast :

Wa Swaheli	Mwezi
Wa Nyamwezi†	
Ma Kwa	
Tette and Sena Tribes ...	
Wa Ganda	
Wajiji	Ukwezi
Wa Chaga	Mwere
Wa Sambala	Muezi
Wa Pokomo	Muezi
Nyassa Tribes	Mwezi.

And on the South-west :

Ora Herero	Omueze
Ora Ambo	
Ba Rotse	Moezi
Angola	Mbeshé
Lubalo, Kisama, &c.	Mbeshi.

But among the greater number of West Coast tribes it is replaced by another word as follows :

Isubu	Ngonde
Diwala	

* I have only come across it in the two following Lake Tribes: Watuta, *inyanga*; Ma Nyuema, *mwango*.

† Wa Nyamwezi, people of Moon land or of the Moon badge. They wear an ivory crescent moon attached to the neck.

Ngoten	}	Ngon
Melon		
Dikele	...	Ngondie
Babenda...	}	Ngonda
Basunda...		
Eshi Kongo	...	Ngondi.

The forms *mwezi*, *unyezi*, &c., are according to Döhne connected with a root *izi*, meaning shining particles.

The various words in use in Kaffir for Star seem to bear the same origin.

Thus star in Xosa is *inkwenkwezi*; in Zulu it is *inkwezi*; while the Xosa for morning star is *ikwezi*, all apparently connected with the Zulu and Xosa *ikwezikwezi*, a glittering substance.

Again we have the Zulu and Xosa *in-Kanyezi*, a fire-fly or star, which seems compounded of *izi* above-mentioned and *nku-kanya*, to "shine or emit light."

A similar word, similarly derived, is the Zulu *ucwazibe*, evening star, which may be compared with *ubu-Cwazicwazi*, brightness.

Another form of *ucwazibe* seems to be the Zulu *icazibe*, evening star, represented in Xosa by *icanziwe*.* Here *zibe or ziwe* seems to be akin to *umsebe*, a ray of light or spreading ray; compare *uku-ziba*, to separate.

Another word of different origin applied by the ama Zulu to the evening star is *isi-celankobe*, from *cela*, meaning beggar, and *inkobe*, corn. But why this star should be called "a beggar for corn" is not apparent.

The root *izi* glitter is seen in *nyenyezi* of the Tette and Sena tribes, *inyeleti* at Lourenço Marques, *nyuladi* at Inhambane, and *menneri* at Quillimane, all with the meaning of star.

In Setshuana stars are called *linaleri*, diminutive *linaletsane*.

Among the ma-Koa they are called *itotwa*, among the wa Swahili, *nyota*.

In otyi-Herero the word for star is *onyose*, and seems to be connected with *nyosa*, "to burn." It is probably the same word as *nyota* of the Swahili just mentioned. A fixed star is *o'nyose ondikame; zikama* meaning "to be fixed"; and a planet is *onyose ondiange*, the latter word from *rianga*, "to rove about." The Morning Star is called *o'hingoutuku*, or "chaser of night," or

* Used to signify Orion by the translators of the Kaffir bible.

okanumaihi, "little drinker of sweet milk," on account of its appearance at milking time.

In Kongo a star is called *ntetembwa*, a term possibly connected, as in Zulu and Xosa, with a word meaning to sparkle or glitter, viz.: *tentena*; and a planet is called *nkazi a ngonde*, the wife of the moon. In Bunda (Angola) a planet is *chi-tetembuka*.

The eshi-Kongo generally call a constellation *ebunda dia ntetembwa*, or "gathering of stars." They have a curious name for the three Stars in the Belt of Orion, namely *mbwe yo nshiji yo nkongwambwa*, or "the dog, the palm rat and the chief huntsman."

In Kafir, Orion's belt is simply *amaroza*, "a line of stars," primarily "a row of beads."

It will be noticed that on the West Coast among the ova-Herero, the eshi-Kongo and the Angolese a distinction is made between fixed stars and planets.

Although generally viewing the heavens with indifference, many Bantu tribes make an exception in favour of the beautiful star cluster around which so many northern legends have gathered.

Miss Clerke tells us that in Arabic the Pleiads are called *Eth Thuraiyâ*, from *therwa*, copious, abundant, because many are gathered in a small space; and that the idea of family or groups is also contained in the Biblical *Kimah*. "Analogy," she says, "then almost irresistibly points to the interpretation of the Greek Pleiads by *pleiônis*, many, or *pleiôs*, full, giving to the term in either case the obvious signification of cluster."

We have seen that "cluster" is one of the meanings attributed to the Nama *Khunuseti*; among the Bantu we find this meaning unmistakably evident in the Herero *oty'ose*. This word is a form of *o'nyose*, star, plural *ova nyose*, and means strictly no more than "starry," "appertaining to stars," "the place of stars." Hence its application among the ova-Herero to the Pleiad Star cluster.

In Kongo the word is *udundalunda*, a swarm, applied also to a swarm of bees. This closely resembles the Australian word *worrul*, a bee's nest applied to the same asterism, and reminds us of the poet's "fire flies tangled in a silver braid."

The eshi-Kongo have, however, another word for the Pleiads *za lunda emvula*, which seems to mean the "storehouse or gathering place of rain." (Cf Inhambane "rainy season" = *nannge-nvula*.) This would appear to connect the Pleiads in the Kongo mind with the advent of the rainy season just as the "pluviæ Hyades" were so con-

nected in classic lore ; and it may be here mentioned that while rain is prevalent all the year around San Salvador, the heavy rains commence about the middle of October.

However this may be, we have evidence that the Eshi-Kongo regarded the movements of the Pleiads as betokening the succession of the seasons ; and this gives weight to that view which would associate the term *Khunuseti*, among the Namaqua, with the arrival of spring.

Stronger proof on this point may, however, be obtained from consulting the vocabularies of the Bantu languages of the South and East Branches.

In Xosa and Zulu the word for the Pleiads is *isilimela*, a term in Zulu bearing the additional signification of "the breaking-up time," *i.e.*, "the ploughing time," and the beginning of spring. Closely connected with this word is the Zulu and Xosa *uku-limela* "to hoe, plough, or break up the land."

The Kafirs would seem therefore, at one time, to have marked the first appearance of the Pleiads as the signal for commencing ploughing operations and as the harbinger of spring.

It is not probable that they applied the term *isilimela* to the Pleiads recently, for their first spring rains and therefore their ploughing season usually commences on the frontier about the beginning of October, while Dr. Gill and Mr. Finlay have very kindly told me that at 32° S. Lat. the Pleiads would be seen rising just after sunset about November the 15th.

Probably therefore the name was first given some centuries ago, when our frontier tribes were in more northern latitudes, before they drove the Hottentots from the banks of the Bashee and Kei.

Perhaps also this word was formed before the South-east Bantu split up into the tribes as we now know them. For the Betshuana use almost exactly the same term for Pleiads, *selemela* and possibly their word for summer, *selemo*, is connected therewith. At Sena, Mozambique, and among the Makoa, *pa-ku-lima*, *ilimue*, *ulima* all have the sense of ploughing or cultivation, but the writer is not aware whether any form of this word is applied by these tribes to the Pleiads.

Among the Wa Swaheli this is the case ; the Pleiads being in Ki Swaheli *Kilimia* ; *Kilimo* meaning cultivation or planted crops, and *Kulimo*, to hoe or cultivate.

A SHORT ACCOUNT OF THE ATTACKS OF THE *TEREDO NAVALIS*, AND *CHELURA TEREBRANS* UPON GREENHEART (*NECTANDRA RODIŒI*) AND SNEEZEWOOD (*PTEROXYLON UTILE*) TIMBERS.

By R. H. Hammersley-Heenan, Mem. Inst. C.E.

[Read 28th November, 1888.]

There is perhaps no experience more remarkable in the abstract, or interesting in its application, than that the greatest enemies of all life—both animal and vegetable—are to be found, in many cases, as organisms, infinitely small, lurking in and feeding on vital tissues, which they soon destroy. A proper understanding of the conditions under which such organisms exist and flourish, is of the vastest importance to applied science in almost every one of its many branches. In surgery the advance made in this direction appears almost incredible.

By the aid of antiseptics the surgeon now fearlessly enters portions of the human body, that a few years ago he would not have dared to touch.

Cultivators of plants now know they have enemies, which although indiscernible to the naked eye, are, when examined under the microscope, found to be armed with instruments of destruction, which they use with merciless rapidity if left undisturbed.

The engineer and naval architect have also long known of one or two of these small enemies. They belong to a class low down in the scale of life, but for all that, they work fearful destruction wherever they find a home. I refer to the *Teredo Navalis*, the *Chèlura Terebrans* and the *Limnoria Terebrans*.

The *Teredo*, or the sea-worm as it is commonly called, is found in wood (under sea-water) which it perforates, nearly always in the direction of the grain of the timber, and lines the tunnel, or cavity, in which it lives with a calcareous wall. These tunnels, and the lined walls, can be easily traced and examined on the specimen pieces of wood I send to illustrate this paper, and the size of the worm itself can be accurately estimated.

The *Chèlura*, and *Limnoria*, are much smaller than the *Teredo*, and perhaps, for that reason are not so much dreaded, but nevertheless they are most destructive, and whatever they may lose in size they make up in numbers. I do not think I have seen the latter in this country. The former however is only too common, as I shall show further on. The specimens marked "B" are a portion of a waling destroyed by the *Chèlura*.

It is far beyond the range of my attainments to give anything approaching a scientific description of these mollusca, as my knowledge of natural history is painfully limited, and even if it were not so, no useful end would be served, for it would be difficult to deal more fully with the subject than M. de Quatrefages has done. I have however to make a statement which, if not scientific, is none the less indisputably and painfully true, and it is this, "the *Teredo* and *Chèlura* work complete destruction in timber that is considered proof against them," as I shall now endeavour to show.

The *Teredo navalis*—as its name implies—has been the dread of ship-builders in times past. And in Holland, and all over Northern Europe the source of endless trouble, oak, pine, elm, and all other kinds of timber then used in Europe succumbed to the ravages of this pest and it was not until the year 1840, when greenheart was first proved to be able to resist the worm in England, that timber could be used with safety in any permanent marine works, unless it was first protected with copper sheeting or scupper nails.

The experiments with greenheart, made some fifty or sixty years ago at Liverpool, having given satisfactory results, this timber at once came largely into general use for marine works, and has since been adopted by engineers in all parts of the world, and although in some localities it has been attacked, still the damage was so slight as to cause but little alarm, and greenheart continued to be considered *Teredo*-resisting timber.

The sneezewood (*Pteroxylon utile*) of this country is another timber that has long enjoyed a reputation of never having been known to yield to the attack of the worm. I shall however have to show in the course of this paper, that both timbers have, within my experience, entirely failed to uphold the favourable reputation they have so long held.

About fourteen years ago the railway from Port Elizabeth to Uitenhage was constructed by a private company, who decided for various reasons to use timber extensively in the construction of

its works. And the Engineer-in-charge who had had considerable Colonial experience, decided to utilize sneezewood for the pile work of all the bridges spanning the numerous small tidal streams that flow into the Zwartkops river; this timber was of the soundest and best quality procurable, and was evidently selected with much care and obtained at no small cost. It was the longest of its kind that I have ever seen, averaging about 20 feet long and about 12 inches square.

Some three years ago, Mr. J. S. McEwen, who was then the District Engineer at Uitenhage, noticed that the piles of a bridge, not far from Zwartkops Station were diminished in section, close to the low water mark. At first he thought it was merely the sap-wood that had decayed, but, on minute examination, discovered that the *Teredo* had attacked the piles, and so reduced their dimensions, that he decided to recommend that they be removed, and iron substituted. This was done, and when the piles were taken out, and carefully examined, it was found that they had in every case been more or less deeply burrowed into for about nine inches above, and nine inches below, the low water mark. I have told these facts to several "old Colonists" of experience in such matters, and in every case have been met by the remark, "it could not have been sneezewood, the worm has never yet been known to touch it." And I fully believe I would have failed to convince them to the contrary had I not been able to produce proof, which there is no possibility of escaping from, in the shape of the timber itself, showing the *Teredo* snugly incased in tunnels of its own making.

I do not think the members of the *South African Philosophical Society* will doubt the honesty of my statements; but as it is reasonable to suppose that they, as a body engaged in original investigation, may assume that I may have been mistaken; and as it is all-important, that no assertion—and more especially one which is opposed to popular belief—should be received until subjected to the most searching investigation, I send a specimen-piece of the timber I refer to—upon which the labours of the worm can be very easily traced—in order that those interested in the subject may examine it for themselves.

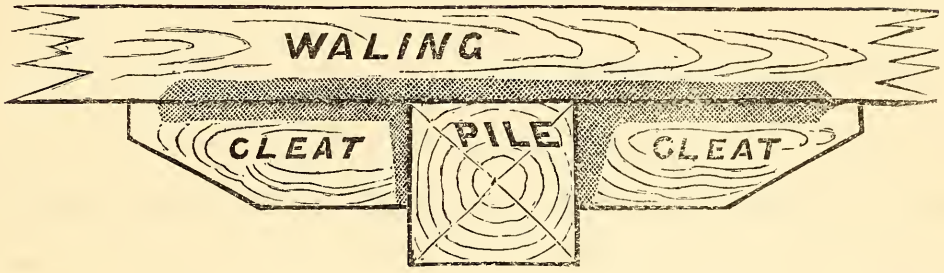
In the first portion of this paper I mentioned that greenheart (*Nectandra Rodiæi*) has long been looked upon in England and Northern Europe as practically a Teredo resisting timber, and that in consequence it has been much used for marine structures, and in most cases with marked success. It so happens however, that it falls to

my lot to show that not only is greenheart attacked by the ordinary worm in this country, but that it is also a victim to the ravages of the *Chèlura terebrans*, and I believe also to the *Limnoria* itself.

About the year 1878 the Port Elizabeth Harbour Commissioners had two Jetties constructed, from the designs and under the professional direction of Sir John Coode, Mem. Inst. C.E. For the most part wrought iron was used, but it was decided that the seaward ends, known as the V heads, should be—for special reasons—of wood, and after due consideration greenheart was selected as the timber most suitable for the purpose, and with this material—excellent of its kind—the work was carried out.

Some time ago, I had to make a detailed inspection of these Jetties, and on examining the V heads, I noticed what at first appeared to be signs of decay in one of the horizontal pieces, and, on cutting into it with an adze, found it completely honeycombed—for about an inch deep, with the tunnels of the *Teredo*, and, on further examination, found that all the horizontal timbers—at low water mark—had been in a like manner attacked. I then looked to the vertical pieces—or piles—and discovered that although the worm had entered many of them, still the progress it had made was small, compared to that on the horizontal pieces.

Having completed my inspection of this portion of the structure, and having satisfied myself that the *Teredo* was in undoubted and secure possession, where it will unquestionably remain until it has completed its work of destruction, I proceeded to examine the timber frame—also of *greenheart*—that has been placed outside the iron portion of the Jetties to act as a fender to prevent boats coming into direct contact with the iron. I there found not only the *Teredo*—evidently thoroughly enjoying itself, but also, in vast numbers, a most destructive little creature which I believe to be the *Chèlura terebrans*, which does not attack the timber in the general and haphazard way in which the *Teredo* appears to do, but invariably commences its work at the ends of the timber, or where one piece comes into contact with another, and in this way—although the actual area destroyed by it is comparatively small—it becomes more injurious to a framed structure than is its great competitor. To more clearly explain what I wish to convey, I give a simple sketch—in plan—showing what I actually found on the day in question. The shading represents the portions destroyed by the *Chèlura*.



It requires but little technical knowledge to see that the "cleats" and "walings" were rendered entirely useless—in fact so useless that at certain points the force of the sea was sufficient to disperse the piles.

Since my first inspection, the result of which I have just endeavoured to describe, I have made numerous examinations of these works, and have watched with interest and dismay the advances made by these mollusca, and am now only too fully convinced that before very long the question of replacing the timber-work will have to be faced, and when I state that it originally cost—for the two Jetties—some twenty-five thousand pounds, the seriousness of my subject may perhaps be realized.

In writing this paper I have had a two-fold object in view, firstly, a desire to let those engaged on similar works as myself know of my experience with timbers that have heretofore enjoyed a reputation which they certainly do not appear to me to deserve. And, secondly, in the hope that some of those members of this Society who have a special knowledge of marine insect life, and time at their disposal, may direct their attention to the subject with a view of discovering some means of guarding against the onslaughts of these most destructive pests. For it appears to me that the age that can boast of the labours of Pasteur on the silkworm, should not allow the *Teredo Chèlura* and *Limnoria* to hold the field and work destruction in the peace and security they now enjoy.

I am aware that attempts have been made in this direction from time to time, and with varying success, but so far as I know nothing has yet been discovered that can be handed to the engineer and ship-builder, with the instructions "Use this, and your timber is safe from marine life attacks," and until that be done there is something *original* for science still to accomplish.

[R. H. Hammersley-Heenan.

SEA LEVELS IN SOUTH AFRICA FROM BAROMETRIC OBSERVATIONS.

By F. Guthrie, LL.B.

[Read 27th February, 1889.]

The table for finding sea level altitudes from barometric observations which is this evening submitted to the Philosophical Society, though calculated according to the ordinary formula $\mu \log \frac{h_1}{h_2}$, is nevertheless empirical because the coefficient μ has not been determined according to theory, but has been assumed, as being 64,300, so as to give correct results in certain cases where these results are capable of independent verification.

The observations on which the table is based are those which have been taken at the Royal Observatory and at Kimberley. Other points where sufficient observations have been taken and where the results can be checked by railway levelling must be regarded as points of verification. Of these the principal are Aliwal North, Cradock, Graham's Town and Port Elizabeth. As the levels calculated by this table and those ascertained by telescopic spirit-levelling agree, within a few feet, the result must be regarded as satisfactory.

At present however the number of such points of verification is too small for thoroughly testing the table, which must therefore for the present be regarded as having only provisional value.

The table is not intended to be used to ascertain sea level from a few barometric observations, it can only be used when a continuous series of daily observations have been taken at a fixed hour.

When such observations have been taken for four or five years at any place, the sea level of that place ought to be deduced from the table with a probable error of less than 5 feet.

If only one year's continuous observations have been taken, the average for that year should be corrected for the average deviation that year from the general average over the Colony, and the result should still be correct within some 10 feet or so.

Even one month's continuous observations with a similar correction should give results sufficiently accurate to be of considerable practical value. As a justification for the abandonment of the theoretical formula in the construction of this table, it may be observed that this formula is based on certain hypothetical conditions which can seldom or never exist.

Thus it is assumed that the difference of pressure at two levels is due to the weight of the intermediate vertical column of air, whereas it is obvious that this will only be the case when the atmosphere is in equilibrium. If there is disturbance of equilibrium, whether of rest or motion, this will clearly affect the tension of the air. The error in this case enters principally in the correction for temperature. Temperatures registered by thermometers near the earth's surface are local and do not represent the temperature of the bulk of the superincumbent air stratum. Thus for example the mean of the temperatures registered by two thermometers, one, say at the Observatory and one on the top of Table Mountain, will not, except by mere accident, give the mean temperature of the stratum of air between these levels. This stratum, coming as it does over vast expanses of ocean, has attained a temperature which can be very little affected by radiation from a few miles of terrestrial surface, whereas the temperature registered by thermometers within a few feet of the surface must be greatly affected by such radiation.

When the points of observation are many miles apart, as for instance in the case of two thermometers, the one at Cape Town and the other at Kimberley, the ordinary correction for temperature at the time of observation is utterly fallacious. Whenever such observations have to be used, better results could probably be obtained by correcting for the mean temperature of the month. Barometric pressure, as is well known, does not in any way obey the daily changes of temperature. Temperature has generally but one maximum and one minimum in the twenty-four hours, while barometric pressure has two maxima and two minima. On the other hand variations of average monthly barometric pressure all seem connected with variations of average temperature.

It has been stated that what is at present required for meteorological purposes is not so much the collection of additional observations as the careful digestion of those which have been already accumulated. There are, no doubt, some good grounds for this observation, but the experience obtained in drawing up this table

rather goes to shew that while we have enough good material from which to draw useful deductions, we have on the other hand a superabundance of bad material from which it is impossible to deduce any satisfactory conclusions whatever. No one who has not made himself acquainted with the facts can realize the difficulty there is in getting a series of trustworthy observations taken under the same conditions at one place, extending over even four or five years.

What is still more unsatisfactory is that some of the fundamental data of meteorology are still undetermined. In drawing up this table, for example, it was necessary to ascertain as exactly as possible the average sea level barometric pressure round the South African coast. This is very imperfectly known. The estimates of sea level pressures at different latitudes, in the best works on meteorology, are very rough and imperfect, and as far as South Africa is concerned it may safely be said that it is only here in the neighbourhood of the Royal Observatory where careful observations have been made for almost fifty years that we have anything approaching accurate knowledge on this subject. Mr. Stone the former Astronomer-Royal of the Cape Observatory calculated this out from the long series of observations taken at the Observatory and deduced a sea level barometric pressure of $30{\cdot}067$ in. with a daily average variation of from $\cdot0214$ in. below that at 4 p.m. to $\cdot0243$ in. above at 10 a.m. There seems some reason to suppose that the average sea level pressure increases very slightly towards Port Elizabeth, and there can be no doubt that there is a very perceptible increase when the coast trends towards the North, as at East London and Durban. The average pressure at Durban is probably about a maximum. Beyond that latitude the known equatorial depression should begin to be felt. What we know nothing about is how far the daily variations which Mr. Stone ascertained to exist here at Cape Town can be applied to other parts of the Colony and especially to inland stations. It may be regarded as some slight argument that the daily variations are substantially the same elsewhere as they are here, that in compiling this table observations taken at different times of the day at different localities have been reduced to 8 a.m. according to Stone's table of reduction, and the result as far as it can be tested is fairly satisfactory.

The delay which has occurred in the publication of this paper in the Transactions of the Society has enabled the writer to make

certain corrections which may add somewhat to its substantial accuracy. In the course of a few years, with the experience which has been learned as to sources of error, there is a reasonable prospect that a considerable increase of accuracy may be attained in these results. Meanwhile what has been done shews that barometric observations, continued over a sufficient space of time, are capable of furnishing very useful results as to sea level. Probably however the approximate accuracy of these results is due to a great extent to the somewhat exceptional climatic conditions of the southern extremity of the African continent. There is no doubt that owing to physical circumstances there is quite an unusual degree of uniformity of average temperature over the region to which these observations apply, and it is this approximate uniformity of average temperature that renders the calculation of sea level from barometric observation as satisfactory as it seems to be. It is very doubtful whether a similar method could be applied with equally satisfactory results to such a continent, for example, as North America, where approximate uniformity of average temperature could not be assumed without leading to entirely erroneous results.

TABLE for calculating Sea Level Altitudes in feet of Places in the Cape, Colony, from average 8 a.m. barometrical readings in inches, corrected for temperature of mercury.

Bar.	Sea Level.	Differ.	Bar.	Sea Level.	Differ.
30·0	75	93	27·0	3017	103
29·9	168	93	26·9	3121	104
29·8	262	94	26·8	3225	104
29·7	356	94	26·7	3329	104
29·6	450	94	26·6	3434	105
29·5	545	95	26·5	3539	105
29·4	640	95	26·4	3645	106
29·3	735	95	26·3	3751	106
29·2	830	95	26·2	3857	106
29·1	926	96	26·1	3964	107
29·0	1022	96	26·0	4071	107
28·9	1118	96	25·9	4179	108
28·8	1215	97	25·8	4287	108
28·7	1312	97	25·7	4396	109
28·6	1409	97	25·6	4505	109
28·5	1507	98	25·5	4614	109
28·4	1605	98	25·4	4724	110
28·3	1704	99	25·3	4834	110
28·2	1803	99	25·2	4944	110
28·1	1902	99	25·1	5055	111
28·0	2002	100	25·0	5166	111
27·9	2102	100	24·9	5278	112
27·8	2202	100	24·8	5390	112
27·7	2303	101	24·7	5503	113
27·6	2404	101	24·6	5616	113
27·5	2505	101	24·5	5730	114
27·4	2607	102	24·4	5844	114
27·3	2709	102	24·3	5959	115
27·2	2811	102	24·2	6074	115
27·1	2914	103	24·1	6190	116
27·0	3017	103	24·0	6306	116

Heights calculated by the foregoing TABLE, from the average Barometric Pressure during the eleven years from 1881 to 1891 inclusive.

Station.	Average Barometric pressure.	Calculated Height.	Height otherwise determined.	Differ.	
Simon's Town ..	30·056	21	...	...	*
Cape St. Francis	30·056	21	20	+ 1	
East London ...	30·044	33	30	+ 3	* (*a*)
Royal Observatory	30·040	37	37	0	*
Cape Agulhas ...	30·009	66	55	+11	(*b*)
Mossel Bay ...	29·963	111	105	+ 6	*
Port Elizabeth ..	29·887	180	181	— 1	*
Dunbrody ...	29·851	214	200	+14	
Clanwilliam ...	29·808	254	245	+ 9	
Wynberg ...	29·811	251	250	+ 1	
Wellington ...	29·640	413	400	+13	
Swellendam ...	29·569	479	475	+ 4	
Oudtshoorn ...	28·928	1081	1063	+18	
K. Wm's. Town	28·683	1329	1314	+15	
Ceres	28·491	1516	1493	+23	
Lovedale ...	28·281	1723	1720	+ 3	
Graham's Town	28·146	1856	1851	+ 5	*
Prince Albert ..	27·887	2115	2120	— 5	
Somerset East ...	27·597	2408	2400	+ 8	
Graaff-Reinet ...	27·486	2519	2500	+19	
Stutterheim ...	27·347	2661	2700	—39	
Cradock	27·169	2845	2856	—11	*
Queen's Town ...	26·445	3597	3543	+54	
Brakfontein ...	26·117	3946	3947	— 1	
Kimberley ...	26·031	4038	4042	— 4	*
Aliwal North ...	25·762	4328	4330	— 2	*
Bloemfontein ...	25·583	4524	4518	+ 6	*
Sutherland ...	25·315	4818	4776	+42	

The results indicated by * are those which have been brought to the test by railway levelling and it will be seen that the average error is about 5 feet on either side. One source of error which the writer has not been able to eliminate is the existence in some cases of undetermined and uncorrected index error in the various instruments used. This will, it is expected, cease to affect the observations made in forthcoming years, since these index errors are now ascertained in almost all the instruments, and no doubt will in future be systematically applied.

Note *a*. For about half the period, the barometer at East London was at a 40 feet level, for the other half at 20 feet.

Note *b*. The true level of the Agulhas Barometer is in doubt, it was estimated a few years ago at 68 feet.

Subjoined is a short TABLE of Places where Observations have been made for not more than four years.

Station.	No. of years.	Average Reading.	Calculated Height.	Height otherwise obtained.	Differ.	
Sea Point ...	1	30·063	16	15	+ 1	*
Port Nolloth...	2	30·026	51	40	+ 11	*
S. A. College	4	29·976	97	115	− 18	*
Stellenbosch ...	3	29·741	318	400	− 82	
Bishop's Court	1	29·856	210	250	− 40	
Storm River ..	2	29·322	714	...	...	
Worcester ...	2	29·251	792	780	+ 12	*
Umtata ...	3	27·709	2294	2400	−106	
Beaufort West	2	27·175	2837	2850	− 13	*
Kokstad ...	2	25·169	4979	4284	+695	

Again it must be remembered that it is only in the case of those lines to which asterisks are subjoined that we have any approach to exact exterior measurement.

The only unsatisfactory line in this table is that which relates to Stellenbosch. An average barometric pressure for the three years 1889, 1890 and 1891, which were in no respect exceptional in their character, ought to have given a fairly close approximation to the true average, and a consequent fairly close estimate of the sea level. An error of over 80 feet is inexplicable, and quite contrary to the experience derived from the rest of the table. It is true that the otherwise determined sea level of 400 feet is probably an approximation by estimate only, but since the railway level is known within a mile or so of the village, it is difficult to believe that any such serious error as 80 feet can have been made. There is, however, obviously an error somewhere. The index error of −·012, which seems to have been applied in part only, will not account for this.

In the case of Kokstad there is obviously some flagrant error somewhere. How such a detailed estimate as 4,284 has been arrived at, it is difficult to say. In the years 1886 and 1887, observations were taken at Kokstad, giving an average pressure of 25·712, and a consequent sea level of 4,383 feet, differing by only 99 feet from the estimate 4,284. Our Kokstad observations are obviously at present unavailable until the source of this gross discrepancy can be ascertained.

It will be seen that a much greater apparent accuracy might have been obtained by leaving out the observations at those places where trustworthy independent means of ascertaining sea level do not exist, as also by omitting some which are vitiated by obvious errors. The real utility of the table would however have been thereby diminished, as it is very instructive to contrast those errors which may fairly be called legitimate, as they arise from the necessary imperfections of our methods, and those which are illegitimate as arising from downright blundering somewhere.

THE RELATION OF THE SAND DUNE FORMATION ON THE SOUTH WEST COAST OF AFRICA TO THE LOCAL WIND CURRENTS.

By H. Carrington Wilmer.

[Read 31st July, 1889.]

The prevailing winds on this coast from the mouth of the Orange River to Walfish Bay are as follows :—

First, in the summer months, namely from November to April, from early morning to midday North to North-west, from midday light South-west, increasing in force till from 7 p.m. to midnight, then dying away gradually and coming round to North before day-break. North to North-west wind brings the fog-bank on shore and sometimes carries it far inland, South to South-east carries it out to sea.

Secondly, winter months, May to October, mornings calm with light North-west to North, fog on land, mid-day to midnight light South-west dying away to calm. Occasional strong northerly wind for from three to four days in succession and once at least in each month for a period of from two to eight days, hot and very dry East wind frequently blowing a full gale. This wind generally comes on about 2 a.m., increasing in force till 10 to 11 a.m., dying away to calm in the afternoon ; it carries clouds of fine dust, consisting, for the most part, of tiny grains of quartz and flakes of mica.

From Angra Pequena to Walfish Bay the coast is lined by a belt of sand hills, varying between five and fifteen miles in width and from 50 to 300 feet high. At the back of these hills is a desert strip from thirty to eighty miles wide, devoid of vegetation and covered by nodules of white quartz and pinkish white felspar. This gravel results from the decomposition of the granite, gneiss and mica schist which form the predominant rocks of the district. As the rocks decompose, the softer constituents, that is to say the mica and the smaller particles of quartz and felspar, are, owing to the want of rain to consolidate them, carried away by the winds, leaving the larger nodules covering the surface, which is consequently white and glaring. These fragments present the appearance of being water-worn, but are

really only polished by the constant attrition of the smaller particles blowin against them. Radiation from this glistening surface and its bordering sand hills is necessarily very great, and as soon as the sun rises the air becoming heated begins to ascend. This action increases as the day advances and the heat becomes greater; and during the summer months, when the air over the sea is much colder and denser than that of the interior behind the desert, a current from the ocean is introduced to fill the vacuum caused by the ascending air. This action, increasing as the radiation increases, and decreasing again as the sun sinks, is the cause of the rise and fall of the South-west wind so prevalent in summer. Again, as all the rain in these parts comes in the form of afternoon thunderstorms from the North-east and falls from November to May, it follows, and actual observation verifies the theory, that these South-west winds day by day drive back the approaching rain clouds, which may be seen banked up and discharging their contents to the eastward where the power of the South-west becomes less as the distance from its generating causes, the desert and the sea, increases. Thus a reciprocal action is in force; the want of rain to check denudation, by the winds, of the decomposing rocks and to promote the growth of vegetation causes the desert, the radiation from which creates the South-west current; while this same South-west wind, by driving back the rain continues the conditions which result in its own formation; consequently there is but little hope of any improvement in the condition of this waste, which is also so saline as to render afforestation or cultivation next to impossible; it will, therefore, probably remain a desert as long as the present geological conditions remain unchanged.

When, as is the case at intervals during the winter months of May to October, the evaporation after the rainy season generates sufficient cold to reduce the atmospheric temperature of the country behind the desert to a point lower than that of the sea, a reverse action is set up and wind from the East rushes in to fill the daily vacuum caused by the rise of the air over the heated desert; and when, as also happens at intervals, the night temperature of the interior falls below the freezing point, coincidently with clear weather near the coast, this wind increases to a powerful gale. Cold at the eastern fringe of the desert it becomes warmer as it passes over the heated surface, till at the coast it is a scorching sirocco. It carries with it enormous quantities of dust resulting from the disintegration

of the desert surface as already described; and as it meets the resistance of the colder and denser stratum of air from the sea, also struggling towards the vacuum, its force gradually becomes exhausted, and, as it dies away, it deposits its suspended particles of sand and mica, partly on the coast and partly in the sea. Some of that which falls into the sea is carried along by the current, which runs strongly to the North, until a submerged reef of rocks is met with as at Sandwich Harbour and Angra Pequena, when a portion of the sand is arrested, the bank thus formed eventually rising above the surface; and thus are formed the sand spits, always trending northwards, by which the bays of Sandwich Harbour and Walfish Bay are protected from Southerly and Westerly winds.

Having thus shewn the effects of the sand dunes and desert on the local winds, we will now consider the relation of these currents to the progressive motion of the sand dunes from North to South. Dr. F. M. Stapff, of Weissensee near Berlin, has, in his report to the German Colonial Society in 1886, concluded that the sand dunes of the South-west coast were formed under the sea and afterwards raised above its level. This is an entirely erroneous conclusion, resulting from cursory and superficial observation, as I will proceed to shew. In the first place the enormous quantities of sand, brought to the coast by the East wind and partially deposited near the shore as already explained, are quite sufficient to account for the extent of the deposit. Secondly, there is a total absence of shells or other organic remains of marine life in the sand of the dunes. Again the sand hills are ever advancing to the North. This is caused by the alternate action of the East wind and the South-west, which is diagonal to the direction of the former, and thus the sand being first pushed to the West and then back to the North-east, gradually extends itself to the North.

In this way the sand hills have, in the course of ages, been advanced to the North extending the desert as they extended, until they reached the Kinsib river. They have completely choked and obliterated the lower courses of several minor rivers, such as the Choondap and others, which now disappear under the eastern edge of the sand dunes. The Kinsib river still sends down sufficient water in years of plentiful rain in the interior to clear its bed of the sand dunes formed in it in the intervals and thus keeps a road from Walfish Bay, where it debouches, open to the interior. The river ran to the sea in 1856, 1864, 1881, and 1885, and the long intervals between gave the sand hills time to

cross it, and form smaller hills as far as the Swakop river. This latter however runs into the sea every year, and scours its bed clean of the sands blown into it during the preceding dry season. Consequently we find no sand dunes north of this river ; and also as we travel northwards and parallel to the coast we find the desert strip steadily narrows until in the Kaoko veldt, about 150 miles north of the Swakop, we reach a district with a steady annual rainfall and grass right down to the sea beach. The struggle which has taken place between the sand and the Kinsib river is beautifully exemplified between Sandwich Harbour and Walfish Bay. There is no doubt that this river formerly debouched at the former port and has gradually been pushed north for thirty miles, until it empties itself at Walfish Bay. This is clearly proved by the old silt bed, with root and stems of reeds still undecomposed, appearing everywhere *under* and between the sand hills, while fresh water is obtainable at a depth of a foot or two, anywhere above high water mark, between Sandwich Harbour and Walfish Bay and nowhere else along the coast ; and the spaces between the sand dunes are covered with water grasses and reeds growing in the silt and nourished by the same fresh water. And in the spaces between the sand hills near Walfish Bay are still to be seen the deeply impressed foot-prints of elephants and rhinoceroses, in the sunbaked silt, thus shewing that prior to the advance of the sand, considerable vegetation, on which those animals subsisted, must have covered the present waste.

I think I have conclusively shewn the great effect which these sand hills have had upon the meteorological conditions of the South-west Coast, and it only remains to point out the lesson to be derived, viz., the great importance of arresting the formation of similar deposits in their early stage by planting them with grasses and shrubs calculated to bind the sand and thus preventing them from extending until they become past control.

H. C. W.